

# Intended vs. unintended consequences of migration restriction policies: evidence from a natural experiment in Indonesia



**Mattia Makovec**

ISER - University of Essex

**Ririn Purnamasari**

The World Bank

**Matteo Sandi**

CEP - London School of Economics

**Astrid Savitri**

The World Bank

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## **Non-technical summary**

International migration has become a global priority in the international development agenda, with three of the seventeen new Sustainable Development Goals including migration related targets. This reflects the remarkable increase in migrants' population in the last fifteen years (41%), but also the recognition that the failure to design appropriate immigration policies in both sending and receiving countries could significantly hinder the benefits of international migration for the world economy.

We study the effects of restrictive emigration policies on economic activity, households' welfare and labor market outcomes in Indonesia, a major origin country of international migrants. In particular, we assess the consequences of restrictions to the international mobility of female domestic workers introduced by the Indonesian government between 2009 and 2011. These were applied to the two most important destinations of Indonesian migrants, such as Saudi Arabia and Malaysia, in response to increasingly frequent cases of abuses and harassment suffered by Indonesian domestic workers in these countries. With female domestic workers representing over 70% of Indonesian migrants before the introduction of the policies, this unique natural experiment has an important external validity.

Our approach exploits the differential impact of migration restrictions (moratoria) across Indonesian villages, and highlights the unintended effects they may have precisely on those they were intended to benefit. The main results suggest that these policies had negative effects on economic activity and households' welfare, and worsened labour market conditions, especially for women, in the origin communities most severely affected.

At the household level, we find a constant decline in per capita consumption expenditure, between 3% and 4% per year, in the first four years after the moratorium to Saudi Arabia. We also find an increase in poverty rate between 2 and 3 percentage points per year in the first three years of the ban to the same destination. Similarly, we find a decline in female employment and labor force participation rates up to 2 and 4 percentage points in the affected areas, in the four years since the first moratorium. Finally, we also find evidence of an increase in enrolment rates in junior secondary school for both males and females in areas more exposed to the moratorium to Saudi Arabia, which could reflect the importance of maternal presence on children's human capital investment.

In conclusion, our results provide novel evidence on the economic consequences of restrictions to migration at the origin, and suggest the importance of international cooperation for the realization of the potential economic and social benefits from international migration.

# Intended vs. Unintended Consequences of Migration Restriction Policies: Evidence from a Natural Experiment in Indonesia

Mattia Makovec\*  
ISER, Essex University  
[mmakovec@essex.ac.uk](mailto:mmakovec@essex.ac.uk)

Ririn S. Purnamasari  
The World Bank  
[rpurnamasari@worldbank.org](mailto:rpurnamasari@worldbank.org)

Matteo Sandi  
CEP, London School of Economics  
[m.sandi@lse.ac.uk](mailto:m.sandi@lse.ac.uk)

Astrid R. Savitri  
The World Bank  
[asavitri@worldbank.org](mailto:asavitri@worldbank.org)

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## Abstract

We study the consequences of a series of migration policies that restricted the migration of Indonesian female domestic workers towards traditional destinations, namely Malaysia and Saudi Arabia. Our difference-in-differences specification exploits the differential impact across Indonesian villages of this unique natural experiment, intended to stop repeated cases of mistreatment of Indonesians working overseas. Our results suggest that the moratoria had negative effects on economic activity and households' welfare, and worsened labor market conditions, especially for women, in the origin communities. Our results highlight the unintended effects that migration restrictions may have precisely on those they were intended to benefit.

JEL Classification: O15, J61, O53.

Keywords: labor migration, migration policy, female migrants, migration restrictions, local labor markets, Indonesia.

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## **1. Introduction and Motivation**

The number of international migrants has increased 41 percent worldwide over the past 15 years, reaching 244 million in 2015, compared with 173 million in 2000. This increase represents a net expansion in the proportion of migrant population, given that over the same period the world population grew at 19 percent (UN International Migration Report, 2015). Migration policies, however, still seem to remain in the domain of national governments in receiving countries, and bilateral agreements between sending and destination countries still represent the exception. This is not surprising, given that for decades migration policies have experienced less cross-country coordination and liberalization compared to trade policies (Hatton, 2007).

Although for decades international migration has remained off the international development agenda (Pritchett, 2006), currently three of the seventeen new Sustainable Development Goals include migration related targets. At least in part, these reflect the recognition that the failure to design appropriate immigration and integration policies in both sending and receiving countries could significantly hinder the large potential benefits of international migration for the world economy (see, e.g., Walmsley and Winters, 2005). The international cooperation in the design of migration policies appears even more relevant in the case of many low- and middle-income countries, that display high emigration rates but also issues of weak implementation capacity, corruption, difficult coordination among government agencies, and poorly regulated labor markets.

In this paper we study the effect of restrictive emigration policies on the economic activity, households' welfare and labor market outcomes of a major origin country of international migrants, namely Indonesia. In particular, we assess the consequences of restrictions to the international mobility of female domestic workers introduced by the Indonesian government between 2009 and 2011. These were applied to the two most important destinations of Indonesian migrants, such as Saudi Arabia and Malaysia, and they were introduced in response to increasingly frequent cases of abuses and harassment suffered by Indonesian domestic workers in these and other Middle-Eastern

countries during the mid- and late 2000s. This context represents a unique large-scale natural experiment, which we exploit to understand the socio-economic and labor market implications of (e)migration restrictions.

Our main research question is whether restricting migration flows at the origin can lead to unintended consequences that could potentially offset the intended benefits. From a methodological standpoint, we adopt two complementary approaches. First, we use a difference-in-differences specification to assess the effects of migration restrictions on economic activity (proxied by the number of household electricity subscribers and the number of small industries at the village level), per capita consumption, poverty and school enrolment. Secondly, we use a synthetic controls approach (proposed by Abadie and Gardeazabal, 2003; and Abadie et. al., 2010) to compare a variety of labor market outcomes between treated regions and a synthetic control group. We analyse the effects of the migration moratoria on employment rate, labor force participation rate and working hours of various groups of people that were likely affected by them.

We find that migration restrictions at the origin have important socio-economic consequences on the local communities. First of all, we show that the number of households electricity subscribers declines by 13% (8%) in the first year following the moratorium in villages characterized by high migration rates towards Saudi Arabia (Malaysia). These negative effects appear larger in rural communities, and seem to persist for various years after the introduction of moratorium. Secondly, at the household level, in districts characterized by high migration rates towards Saudi Arabia, we find a constant decline in per capita consumption expenditure, between 3% and 4% per year, in the first four years after the moratorium. We also find an increase in poverty rate between 2 and 3 percentage points per year in the first three years of the emigration ban. Third, our synthetic control analysis suggests also a worsening of local labor market conditions, especially for low-educated women, measured by a decline in employment and labor force participation rates between 2 and 4 percentage points in the affected provinces between 2009 and 2011.

Interestingly, we also find evidence of different types of responses to the labor supply shock induced by the moratoria. In particular, we find evidence of a slight increase (in the order of 10%-11%) in the number of small industries in villages that send migrants to Saudi Arabia, although less robust to alternative specifications. Finally, we find an increase in enrolment rates in junior secondary school for both males and females in areas exposed to the moratorium to Saudi Arabia. Arguably, this could reflect the importance of maternal presence on children's human capital investment. Our results provide novel evidence on the economic consequences of restrictions to migration at the origin, and suggest the importance of international cooperation for the realization of the potential economic and social benefits from international migration.

The paper is organized as follows. Section 2 presents the existing literature and our element of contribution. Section 3 illustrates the Indonesian policy context and the introduction of the moratoria. Section 4 discusses our empirical strategy. Section 5 presents the results based on the difference-in-differences estimation, while Section 6 discusses the results of the synthetic controls analysis. Section 7 summarizes the main conclusions.

## **2. Relevant Literature**

Although the population of international migrants increased markedly over the past 15 years, actual international migration flows are still relatively small in size compared with the total world population: the foreign-born population constituted only 3.38 percent of the world population in 2015 (UN International Migration Report, 2015), and 10 percent of the population in OECD countries in 2010 (Ortega and Peri, 2015). Restrictive immigration policies in receiving countries are often indicated as the main reason for low international mobility (see, e.g., Pritchett 2006). Since restrictions to migration are generally imposed by recipient countries, it is not surprising that the majority of studies has focused on restrictions to (im)migration at destination. Focusing on immigration restrictions, Ortega and Peri (2012) document that, on average, migration restrictions decrease immigration by 6 percent among rich countries. Some recent studies have also attempted

to measure the unrealized economic gains due to excessively restrictive immigration policies, generally concluding that these gains may be very large. Klein and Ventura (2009), Clemens (2011), and Di Giovanni et al. (2015), are some of the studies that document large potential gains from liberalizing international migration. Facchini and Mayda (2009), Boeri (2010), and Facchini et al. (2011), complement this literature by providing evidence on the economic and political forces that determine the formation of the immigration policies of the host countries.

This study contributes to a number of strands of the migration literature. First, we contribute to the recent literature that examines the consequences of migration policies by focusing on an unusual restriction to emigration imposed at the origin by a major net exporter of international migrants. Due to the rarity of these interventions, the migration moratoria implemented in Indonesia since 2009 provide a valuable opportunity to assess the consequences of a large-scale restrictive migration policy at the origin. With this respect, our contribution is aligned to the recent work by Dinkelman and Mariotti (2016), who show the long-run positive consequences of emigration on human capital accumulation in sending communities. The authors exploit two exogenous policy shocks which generated first an expansion, and then a sudden and permanent drop in the flows of Malawian workers recruited by mining companies in South Africa: the removal of an existing quota on Malawian workers in South Africa in 1967, and a permanent labor ban dictated by the Malawian President, after a plane crash killed 70 returning migrants in 1974.

Secondly, this study provides an original contribution to the literature on the effects of emigration on the origin labor market, by focusing on a context where migration is predominantly a female phenomenon. Until recently, very few papers have looked at the effects on local labor markets of emigration in the countries of origin (Hatton, 2014), and this literature has mostly focused on the effect of emigration on non-migrants' wages. The general conclusion that emerges from this literature is that emigration is likely to exercise upward pressure on wages due to the reduced supply of labor in the origin market. Nonetheless, in most of the contexts examined in the literature,

migrants are often *males*. Taylor and Dyer (2009), in their simulation from rural Mexico, conclude that emigration impacts positively on wages in origin communities where transactions are frequent among households. Mishra (2007) finds that emigration from Mexico in the 1990s caused the relative wage of high-school graduates to increase by 4 percent, and the wages of those who completed college by 3 percent. Evidence from Mexico of a positive effect of emigration on local wages is also presented by Aydemir and Borjas (2007) and by Hanson (2007b). Borjas (2008) finds similar effects for Puerto Rico, and Bouton et al. (2009) for Moldova. Other studies exploring the effect of emigration on wages of non-migrating nationals are Dustmann et al. (2012), and Elsner (2013a, 2013b). Airola (2008), Amuedo-Dorantes and Pozo (2006), and Hanson (2007a and 2007b), present evidence that labor supply decreases in Mexico as a result of migration. Acosta (2006 and 2007) finds similar conclusions from El Salvador, while Damon (2009) shows additional evidence from rural El Salvador that the effect of foregone labor induced by emigration may actually increase on-farm labor hours for all family members and substantially reduce hours of off-farm labor for males.

We contribute to this literature by assessing the consequences of the recent migration moratoria for female domestic workers in Indonesia on origin labor markets. Since international migration in Indonesia is predominantly a female phenomenon, the recent experience of Indonesia provides an interesting case study to analyse the effect of female migration on origin labor markets. Given that important gendered patterns may emerge in the labor market effects of emigration, this is not of secondary importance.

Finally, we also contribute to the literature that studies the effect of migration on the so-called ‘care-drain’ and family disintegration. Parental absence is often found to generate significant effects on health, education, social relations and family cohesion in the origin household (for a review of the literature, see Antman, 2013). Ehrenreich and Hochschild (2003), D’Cunha (2005), Oishi (2005), and Fudge (2010), are some of the studies that describe the socioeconomic and political



factors pushing women from developing countries to emigrate and work as carers for children or the elderly in richer countries. In the case of Indonesia, Nguyen and Purnamasari (2011) find that female migration does not lower labor-force participation of remaining household members as they do in the case of male migration. Moreover, they find that international migration reduces child labor supply in households with female migrants. Using three different sources of data on labor markets in Indonesia, as well as a novel identification strategy, we present new evidence on the effects of female migration on a broad range of outcomes in the origin household, such as the level of per capita consumption, the poverty status, and the school enrolment of children left behind.

### **3. Policy Context: Restrictions to International (E)migration in Indonesia**

International migration is a major phenomenon in Indonesia and the positive effects of international remittances on the reduction of poverty and child labor have been recently documented (e.g., Adams and Cuecuecha, 2010; Nguyen and Purnamasari, 2011). In 2014, remittances reached 1 percent of national GDP, showing a higher incidence on the economy than government expenditure on social assistance programs.

Before the introduction of migration restrictions, Indonesian migration was largely driven by female migration. As shown in Figure 1, according to administrative data on annual documented flows, in 2006, female migrants accounted for 80% of total documented migrants (BNP2TKI). In the same year, female migrants to Saudi Arabia and Malaysia, the two top destinations of Indonesian migrant workers, represented 55% of total (documented) flows, and 70% of total female migrants. These figures represent the result of a rapid “feminization” of migration flows in Indonesia during the previous decade, most likely spurred by an increasing demand for domestic workers from the Middle East and from neighboring countries. In less than ten years, the female share on total documented migrants rose from 56% in 1996, to 68% in 2000, reaching 78% in 2004 (IOM, 2010).

The statistics from BNP2TKI, unfortunately, do not allow a more detailed breakdown of documented flows by migrants' characteristics. However, data from the recent World Bank Survey on Indonesia International Migration and Remittances (WB-IMR Survey, World Bank, 2014a), show that over 72 percent of Indonesian migrants come from rural areas of the country. Further, the same source confirms that Indonesian women overseas are predominantly employed as domestic workers (representing over 80% percent of total female migrants), as opposed to men, mostly employed in agriculture and construction (around 70% percent of total male migrants). The WB-IMR Survey also shows that female migrants are mostly low-skilled, with 50% possessing only primary education, and 30% junior secondary education.

Emigration in Indonesia is a complex and lengthy process, on paper highly regulated, involving a number of intermediaries and several administrative steps (in Appendix 1, we identify at least 22 procedures required before departure). In order to be able to work overseas, Indonesian workers need to apply for a KTKLN card, a special ID card for migrant workers obtainable only with a job offer from a foreign employer. Migrants can apply at local Manpower Offices (Disnaker), or at BNP3TKI offices (local branches of BNP2TKI). However, in rural areas, where these facilities are not always available, prospective migrants are more likely to apply through local agencies or informal sponsors. Often migrants lack complete information about the job offered and their prospective employer, since the intermediation is usually carried out between migration agencies in Indonesia and their counterparts at destination; at the same time, the heterogeneity in the quality of agencies is high (IOM, 2010). Further, even in the presence of a written job offer, substantial contractual rights might be excluded or under-enforced. Despite the complex *de jure* regulation in place on paper, *de facto* migrants are therefore exposed to risks at all stages of the migration process. Further, claims of abuse and exploitation appear common, ranging from unpaid wages to inadequate rest and physical abuse (Farbenblum et. al., 2013, and IOM, 2010).

In an attempt to respond to these challenges, and to a series of tragic cases involving tortures, murders and death sentences experienced by Indonesian female domestic workers, the Indonesian government imposed a moratorium on migration of female domestic workers from Indonesia to Malaysia in June 2009, and to Saudi Arabia in June 2011. Placement countries on which moratoria also apply include Kuwait from September 2009, Jordan from July 2010, Syria from August 2011, the United Arab Emirates from October 2013, and Qatar from November 2013. In 2015, the migration ban was extended to 21 countries across the Middle East, North and East Africa, and Pakistan (Ministry of Manpower, and BNP2TKI).

The announcement of the emigration ban to Saudi Arabia in June 2011, was triggered by the execution of Ruyati Binti Sapubi, an Indonesian maid who killed her employer's wife after suffering repeated abuses. After the announcement of the ban, the Saudi government in turn announced the suspension of work permits to Indonesian domestic workers (The Economist, 2011). It is therefore likely that this moratorium, which entered into force in Indonesia in August 2011, had been binding for the intended target group. The circumstances were similar to those triggering the 2009 emigration ban to Malaysia. However, in December 2011, the moratorium to Malaysia was lifted, after the two governments signed a Protocol aimed at improving the living and working conditions of migrant workers (Hickey et al., 2013, and ILO, 2016).

Coordination problems and duplications of functions between the Ministry of Manpower and BNP2KI (Farbenblum et. al., 2013), might have hindered the full enforcement of migration restrictions, however in a way which remains difficult to quantify empirically. Furthermore, it remains unclear, a priori, to which extent undocumented migration flows might have been affected by the moratoria. Undocumented migration, in fact, is an important phenomenon in Indonesia, but at the same time difficult to estimate. Undocumented flows are not included in BNP2TKI administrative data, and in areas of low compliance with government regulations, undocumented flows may not have been affected. For instance, the Strait of Malacca between Sumatra and the

Malay Peninsula is known to be a channel through which undocumented Indonesian migrants, typically facilitated by a local *taikong*<sup>1</sup>, reach Malaysia to work informally without contract or protection (see Human Rights Watch, 2004, Kaur, 2004, Wong, 2005, and IOM, 2010). These type of flows might be less likely to be discouraged by the restrictions introduced. A recent report by the World Bank (World Bank, 2016), argues that in some cases undocumented migration might have even increased, as a response to the moratoria. On the other hand, in areas characterized by better compliance with migration regulations (e.g. because of a larger presence of formal recruiting channels), undocumented flows might have been further discouraged. Given the challenges associated to the measurement of undocumented migration, our preferred reduced form estimates of the first-round effect of moratoria on migration flows, remain based on documented flows. The next section discusses in detail how this information is relevant to our empirical strategy.

#### **4. Identification Strategy and First Order Effects of Migration Restrictions**

Figures 2A, 2B, 3 and 4 provide descriptive evidence of the effect of the moratoria to Saudi Arabia and Malaysia on documented migration flows. Figure 2A shows that between 2010 and 2011 (when the moratorium to Saudi Arabia was first enacted), the yearly flows of female migrants dropped from 203,625 to 110,641 units; in 2012 this number plunged further to only 18,356 units. In the case of Malaysia, given the relative importance of male migration to this destination, the drop in the yearly flows of female documented migrants appears less substantial in absolute terms, but remains sizeable. At the end of 2008, the yearly flows of documented female migrants to Malaysia amounted to 102,145 units, while by the end of 2011 this number had fallen to only 38,122. Weaker enforcement capacity in regions predominantly sending migrants to Malaysia might explain why, after the introduction of the moratorium, the flows of documented female migrants did not fall more dramatically. Further, the removal of the moratorium after 2011 might explain the slight recovery of female migration to Malaysia. Figure 2B shows that, given the overall importance of female

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<sup>1</sup> Term used in Indonesia to identify a smuggler, or an informal/illegal sponsor (Kaur, 2004, and Wong, 2005).

migration in Indonesia, also the trends in total documented migration flows resulted heavily affected by the moratoria.

As shown in Figure 2A and 2B, alternative migration destinations did not act as perfect substitutes for female domestic workers. Other Middle East countries in fact were facing similar challenges of abuse and harassment, and were themselves subject, with different timings, to migration restrictions. Migration flows towards “new” destinations (such as Singapore, Taiwan and Hong Kong) gradually increased over time, but only partially compensated for the dramatic drop in flows to Saudi Arabia and Malaysia. Also, due to more demanding educational requirements in Singapore, Taiwan and Hong Kong, it would have been very difficult for all domestic workers subject to the restrictions, to choose an alternative destination in response to the moratoria. As shown in Figure 3, male migration did not fully substitute either for the drop in female migrants caused by the restrictions.

Since it is not possible to identify exactly in the administrative data the number of domestic workers by gender and destination country, in Figure 4 we use information on placements by job type at destination. According to the classification of BNP2TKI, “informal jobs” at destination would correspond mostly to maids and domestic workers. In line with the previous charts, Figure 4 shows that the share of informal placements halved following the moratoria, dropping from over 83% in 2009 to 42% in 2014. In sum, this evidence suggests that the restrictions imposed by the moratoria were indeed binding for most of the perspective female migrants to Saudi Arabia and Malaysia, at least in the short run.

For most of our empirical analysis of the first order effects of moratoria on migration flows, we use data from the *Podes* village census. The *Podes* village census includes information about village geographic characteristics, infrastructure, political participation, main sources of economic activity, and number of village residents working abroad as documented migrants (TKI) during the survey

year. Moreover, in 2005, *Podes* also collected information on the main destination country for people emigrating from each village. Further, in the same year, the gender breakdown of the total number of migrants was also collected. The information is reported by the Head of the village, and is based on administrative evidence. Further, the fact that the National Statistics Office (BPS) fully validates the statistical information included in each wave of the village census, reinforces the external validity of the statistics on documented migrants in *Podes*. For these reasons, *Podes* data are known to match well the aggregate number of documented migrants reported by BNP2TKI, as well as those obtained from national household surveys (Bazzi, 2012).

We are therefore able to identify in the *Podes* census the villages that are sending migrants mostly to Saudi Arabia, or to Malaysia and other destinations, as well as villages that are not migrant senders. Villages that are mostly sending migrants to Saudi Arabia and Malaysia are therefore considered as “treatment” villages, which are more likely to be affected by the policy. In our econometric analysis, we use this information to exploit the spatial variation in the impact of the moratoria. We combine variation in space and time in our data to estimate a difference-in-differences specification.

Tables 1A and 1B compare average characteristics of villages exposed to moratorium to Saudi Arabia (Malaysia) with all other *Podes* villages in 2005, including balancing tests. They show that villages sending most of their migrants to Saudi Arabia (from now on, labelled for the sake of brevity “Saudi villages”) are more populated, more rural and seem to be characterized by higher occurrence of crime than other villages. No significant differences emerge in the main economic activity of Saudi villages compared to other villages. “Malaysia villages” have instead similar population compared to control villages, are also characterized by higher crime occurrence, but their main economic activity is more likely to be agriculture, and less likely industry and retail. Since we estimate a difference-in-differences specification, these differences in observable

characteristics between treatment and control villages in the pre-treatment period are not a threat to our identification strategy, as long as they remain constant over time.

In the regression analysis, we use a difference-in-differences specification to assess the effect of the moratoria on the number of migrants and on two outcomes capturing economic activity at the village level, namely the number of households electricity subscribers, and the number of small industries (below 20 employees). The use of the number of households electricity subscribers as a proxy for economic activity is in line with similar measures recently proposed by the literature, such as nighttime light intensity (see Bazzi et al., 2016).

In their functional form, our difference-in-differences specifications can be expressed as follows:

$$Y_{it} = \beta D\_SaudiVillage_{i,2005} + \gamma' D\_year_t + \delta'(D\_year_t * D\_SaudiVillage_{i,2005}) + X_{it}'\theta + \mu_i + \varepsilon_{it} \quad (1)$$

$$Y_{it} = \beta D\_MalasiaVillage_{i,2005} + \gamma' D\_year_t + \delta'(D\_year_t * D\_MalasiaVillage_{i,2005}) + X_{it}'\theta + \mu_i + \varepsilon_{it} \quad (2)$$

where  $Y_{it}$  represents the outcome of interest (log of the number of migrants, log of the number of households electricity subscribers, and log of the number of small industries, respectively in each equation),  $i$  stands for the village, and  $t$  represents *Podes* survey years: 1999, 2002, 2005, 2008, 2011, and 2014. The dummies  $D\_MalasiaVillage_{i,2005}$  and  $D\_SaudiVillage_{i,2005}$  do not vary over time and indicate whether the village in 2005 was sending migrants mostly to Malaysia or Saudi Arabia, respectively. Villages in which most migrants emigrate to Malaysia (or Saudi Arabia) in 2005, i.e., before the announcement and the subsequent introduction of the moratoria, are therefore considered as the “treatment group” affected by the policy. Although the moratoria were implemented at the national level, due to the importance of established migration networks in the migration decision (Munshi, 2003; Beaman, 2012), it is plausible that these villages were more exposed to the migration restrictions implied by the moratoria. Therefore, if the moratoria had any effect on the origin communities and their labor markets in Indonesia, we would expect this effect

to be concentrated among these villages.  $D\_year_t$  is a vector of time dummies,  $X'$  is a vector of control variables including the constant term, and  $\mu_i$  is a village fixed effect. The vector of coefficients  $\delta'$  on the interaction terms ( $D\_year_t * D\_MalaysiaVillage_{i,2005}$ ) and ( $D\_year_t * D\_SaudiVillage_{i,2005}$ ) represents therefore the difference-in-differences estimator of the effect of moratoria to Malaysia and Saudi Arabia, respectively, on the dependent variable of interest.

The announcement of the moratorium to Saudi Arabia in June 2011, followed an escalation of protests occurred in the previous months against cases of mistreatment of Indonesian domestic workers. For instance, in April 2011, the initial three-year sentence against a Saudi employer accused of torturing an Indonesian maid, was overturned by the appeals court, generating public outcry in Indonesia, as reported by local and international media (BBC, 2011a and 2011b). Given this context, we chose 2011 as the first “treatment” year in the specification of equation (1). By the same token, we chose 2009 as the first “treatment” year in the specification of equation (2).

Table 2 shows the results of the estimation of the first order effects of the moratoria to Saudi Arabia and Malaysia on documented migration flows. The coefficients on the interaction terms between the dummy “Moratorium” and the dummy “Saudi village” (or “Malaysia village”), provide evidence of a very strong first order effect, robust to different specifications including district-specific time trends and time-varying village controls. These results confirm the initial graphic impression that migration restrictions reached their intended outcomes of reducing dramatically migration flows in exposed villages. This result is important as it shows that different geographic areas were exposed differently to the moratoria. This, in turn, justifies our use of a difference-in-differences reduced-form specification in the rest of paper.



## 5. Results

### a. Effects of the moratoria on the local economic activity

Having documented the strong and spatially-heterogeneous impact of the moratoria on the international migration flows of Indonesians, in this section we present the results from the estimation of equations (1) and (2) on our measures of local economic activity, namely the number of electricity subscriber households and the number of small industries in the village. The results of equation (1) are reported in Tables 3A and 4A, while the results from equation (2) are reported in Tables 3B and 4B. In all cases, to test for the existence of differential pre-treatment trends between treatment and control villages, we define an event study setting where we allow for separate treatment effects in each pre- and post-moratoria year. Column [1] shows our baseline specification. In column [1], the control variables included are: a dummy indicating whether the village is rural (*desa*) or whether it is located in a more urban area (*kelurahan*); the main source of income in the village (agriculture, mining, industry, retail, or other sector); some characteristics of the head of the village (level of education, a quadratic function of age and gender); the number of villages in the district (*kabupaten/kota*) where the village is located, and the share of urban villages in that district<sup>2</sup>. In column [2] we also add controls for the number of kindergartens, the number of primary schools, and the number of high schools in the village, as time-varying proxies of infrastructure at the village level<sup>3</sup>. Furthermore, in columns [3] and [4] of each table, we present the results of the estimation restricted to rural villages only (*desa*), which represent over 80 percent of the *Podes* census. Time fixed effects and village fixed effects were included in all our estimates; since a number of controls were calculated at the *kabupaten/kota* (district) level, we clustered robust standard errors at the district level.

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<sup>2</sup> We include controls on age, gender and level of education of the village head since s/he is the respondent of the questionnaire, and because these variables may also capture wider socio-economic components at the village level.

<sup>3</sup> We could not include controls for health facilities (e.g., on the number of hospitals, Puskesmas, and facilities for vaccinations of children in the village) because information on these variables is not available in all years in the *Podes* surveys.

Table 3A shows the effect of the moratorium to Saudi Arabia on the number of electricity subscriber households, while Table 4A shows the effects of this moratorium on the number of small firms. In Tables 3A and 4A, we show the results of our flexible specification estimates, where we allow for heterogeneous treatment effects for pre- and post-moratoria years. In particular, we report event study estimates for four pre-moratoria coefficients  $\delta$ 's:  $(D\_year_{2002} * D\_SaudiVillage_{i,2005})$ ,  $(D\_year_{2005} * D\_SaudiVillage_{i,2005})$ ,  $(D\_year_{2008} * D\_SaudiVillage_{i,2005})$ , with  $(D\_year_{1999} * D\_SaudiVillage_{i,2005})$  as an omitted category. Also, we report two post-moratoria  $\delta$ 's in bold:  $(D\_year_{2011} * D\_SaudiVillage_{i,2005})$ ,  $(D\_year_{2014} * D\_SaudiVillage_{i,2005})$ .

The estimates for the pre-moratoria  $\delta$ 's in Tables 3A and 4A show evidence of no time-varying differences between treated and control villages prior to the migration ban to Saudi Arabia. Coefficients on these interaction terms are not significantly different from zero, in support of the hypothesis of no pre-moratoria systematic differences in trends between the treatment and control villages. This conclusion is robust to the inclusion of varying sets of controls at the village level (column [2]), and also holds when we conduct the analysis on rural villages only (columns [3] and [4]).

The estimates in Tables 3A and 4A of the two post-moratoria  $\delta$ 's  $(D\_year_{2011} * D\_SaudiVillage_{i,2005})$  and  $(D\_year_{2014} * D\_SaudiVillage_{i,2005})$ , can therefore be interpreted as the effect of the moratorium to Saudi Arabia on the variable of interest in the villages exposed to moratoria. In the first year of the moratorium, columns [1] in Tables 3A and 4A show an impact of the moratorium on both the number of electricity subscriber households and the number of small industries in the village. In both cases, the estimated effect is significant, and it rises by 2014: the number of electricity subscriber households declines by 13.3% in 2011 and by 15.1% in 2014 compared to the pre-moratorium period, in the villages affected by the moratorium compared to other villages. In rural villages, these effects are stronger. Interestingly, while the effect of the migration ban to Saudi Arabia on the number of electricity subscriber households is negative, a

positive effect appears on the number of small and medium industries. However, this latter effect does not seem robust to the inclusion of controls for the availability of schools in the village.

The results in Table 3A appear robust to equation specification and sampling restriction, and they suggest that the moratorium to Saudi Arabia resulted in a decrease of economic activity in villages more directly exposed to it. This may be due to an impoverishment of the local population, due to the simultaneous loss of remittances from migrants and to the inability of the local market to absorb the excess supply of labor in the short run. It is also possible that the results in Table 3A reflect the internal relocation of Indonesian households, that may have responded to the moratorium by migrating internally. Although the outcome variables available at the village level do not allow us to answer these questions, in the following sections we investigate these issues by assessing the impact of the moratoria both at the household level, i.e., on per capita consumption, poverty and school enrolment, and at the labor market level, i.e., on the employment, labor force participation rate and hours worked, measured at the province level.

Tables 3B and 4B show our event study estimates for the moratorium to Malaysia. We still include in the analysis four pre-moratorium coefficients  $\delta$ 's, and two post-moratorium  $\delta$ 's. The estimates for the pre-moratorium  $\delta$ 's in Tables 3B and 4B show no pre-treatment differences in trends between the treatment and control villages. In columns [2], [3] and [4] of Table 3B, a significant coefficient at 10 % level appears associated to the  $(D\_year_{2002} * D\_MalaysiaVillage_{i,2005})$  dummy. This suggests that, in villages that send migrants predominantly to Malaysia, the number of electricity subscriber households decreased in 2002 compared to control villages. Given that no significance appears in the coefficients of the following pre-moratorium years (2005 and 2008), we do not regard this as sufficient evidence to invalidate our difference-in-differences estimation.

Following the introduction of the moratorium to Malaysia, we find a significant decrease (by 8.2%) in the number of electricity subscriber households in treated villages, with the estimated effect

becoming larger (-9.3%) and strongly significant in statistical terms by 2014. Again, stronger effects are found in rural villages (Table 3B). In contrast, as shown in Table 4B, the moratorium to Malaysia did not impact on the number of small and medium industries in the village. Both conclusions appear robust to equation specification and sampling restriction.

Finally, we also augment our event study estimates by allowing heterogeneous effects by migration intensity in sending villages. By doing so, we take into account that migration rates are fairly heterogeneous across villages affected by the same type of restriction, with some villages featuring particularly high rates of migration compared to the average. Thus, one could expect villages characterized by higher sending rates to be more strongly influenced by the moratoria, and in turn worry that the findings discussed above may be driven by a small subgroup of villages with high migration rates. To test for these heterogeneous effects, we re-estimate equations (1) and (2) using a continuous treatment approach. Under a continuous treatment specification, we interact all the terms in our event study setting with a continuous variable that captures the heterogeneous intensity of the treatment (i.e., the migration moratoria). To measure heterogeneity in migration intensity across villages, we use the female migration rate calculated at the village level from the *Podes* census in 2005. We calculate this term using data from 2005 to ensure that this measure is not contaminated by the moratoria, nor by any anticipation effects. For each migration moratoria, we then interact this term ( $Mig\_intensity_{i,2005}$ ) with all the pre-moratoria and post-moratoria  $\delta$ 's in our event study setting. Formally, our continuous treatment specification of equation (1) becomes:

$$\begin{aligned}
 Y_{it} = & \beta D\_SaudiVillage_{i,2005} * Mig\_intensity_{i,2005} \\
 & + \gamma' D\_year_t * Mig\_intensity_{i,2005} + \delta'(D\_year_t * D\_SaudiVillage_{i,2005} \\
 & * Mig\_intensity_{i,2005}) + X_{it}'\theta + \mu_i + \varepsilon_{it} \quad (1')
 \end{aligned}$$

The distribution of female migration rates for both “Saudi” and “Malaysia” villages is shown in Figures 5A and 5B, respectively. The results of the estimation based on the continuous treatment specification are reported in Table 5A for the moratorium to Saudi Arabia, and in Table 5B for the

moratorium to Malaysia. In both tables we show both estimated coefficients for all the pre-moratorium and post-moratorium  $\delta$ 's, and their interactions with the measure of migration intensity at the village level. In both cases, the effects of the moratoria on the number of electricity subscriber households do not appear to increase with the intensity of migration. This is explained by the fact that our non-interacted pre-moratoria and post-moratoria estimates remain substantially unchanged compared to our main results in Tables 3A to 4B. This conclusion applies also to the analysis of the effect of the moratoria on the number of small industries in the village. In sum, these estimates suggest that our main results are unlikely to be driven by a few villages with particularly high migration rates, but rather they seem to capture the generalized effects of the moratoria on the villages that used to send migrants to Saudi Arabia and Malaysia, respectively.

**b. Effects of the moratoria on consumption, poverty and schooling outcomes**

The results in the previous section provide evidence that the migration restrictions enacted by the Indonesian government affected negatively the economic activity in origin communities. In this section, we complement our analysis using individual level data from the household survey *Susenas* (Socio-Economic Survey of Indonesia) to examine the mechanisms underlying these effects. In particular, we study the effect of the moratoria on households' welfare, measured by per capita consumption, poverty status, and the enrolment of children in schooling age (in primary and junior secondary school, for both males and females separately).

A plausible explanation for the slowdown in economic activity in the villages affected by the moratoria may be a simple income effect: the migration ban might have resulted in a fall in migrant women's labor income and in a drop in remittances, which in turn may have induced a reduction in household consumption. By preventing access to financing from remittances from overseas, the migration ban might have also affected the poverty status of migrants' households: this is an

outcome of primary interest, given that international migration represents a traditional strategy of poor rural households to escape poverty and sustain their origin families.

Information from the *Susenas* on school enrolment of young children also allows us to examine the effect of maternal presence induced by the moratoria on children's schooling outcomes. This may contribute to our understanding of the slowdown induced by the moratoria, because a negative income shock deriving from the lack of remittances might have as well induced an unusual school dropout behavior of pupils. However, this natural experiment allows us to explore more generally the effect of the maternal presence on children's educational attainment. Most of the related literature has explored the effect of parental absence on educational outcomes of children left behind in contexts in which migrants are men, examining, *de facto*, the consequences of the absence of the father when he becomes a migrant. Cox-Edwards and Ureta (2003), Yang (2008) and Alcaraz et al. (2012), are some recent studies that document a positive impact of emigration on the educational achievement of children left behind, mostly through remittances. Antman (2011) provides evidence from Mexico that spousal control over the intra-household allocation of resources is a major mechanism through which parental migration may affect children: while women have a greater control over the decision-making process of the household while the father is abroad, resources shift back to boys once the father has returned. Finally, a number of studies find evidence of detrimental effects of parental absence on the education of children, including Zoller Booth (1995) from Swaziland, Lahaie et al. (2009) from Mexico and Giannelli and Mangiavacchi (2010) from Albania.

For this analysis, we use all the available survey years of the *Susenas* from 2002 until 2014 inclusive. The *Susenas* is a survey regularly conducted by BPS-Statistics Indonesia to collect information on consumption, housing conditions, social benefits, demographics, employment, education and other socioeconomic characteristics of households. It is representative at the *kota/kabupaten* (district) level, and is the main source of information for the calculation of official

poverty and inequality statistics by BPS-Statistics Indonesia. In order to identify “treatment” districts exposed to the moratoria in the *Susenas*, we resort again to the *Podes* census. First, we identify, in *Podes*, districts with higher female migration rates to Saudi Arabia and to Malaysia compared to other destinations in 2005, by aggregating at the district level, village-level data on female migrants and main destination. Then, we merge *Podes* districts with *Susenas* districts using common identifiers. With this information available, we can adopt a difference-in-differences specification in all similar to the approach outlined in Section 5a. Availability of 13 repeated cross-sections of yearly data at the individual level, allows us to check for differential treatment effects in both pre-moratoria and post-moratoria years over a longer time period.

Tables 6A and 6B presents our estimated effects of the moratoria to Saudi Arabia and Malaysia, respectively, on our household-level outcomes of interest. Since the *Susenas* survey is representative at the district level, all estimated equations include district fixed effects. The first two columns of Table 6A show the estimated effects of the moratorium to Saudi Arabia on per capita household expenditure and on the poverty status (a dummy indicating whether an individual lives in household whose per capita expenditure falls below the poverty line). The results show no significant difference in pre-moratoria trends until 2009. In 2010, we find poorly significant (at 10%) pre-existing differences between “treatment” and “control” villages in the consumption equation, and a strongly significant pre-existing difference in the poverty equation. The coefficient on the 2008 dummy is dropped in both columns [1] and [2], as data on per capita expenditure were excluded for that year for data quality issues (see World Bank, 2014b)<sup>4</sup>.

In the first year of the migration restriction, we find a decline in per capita household expenditure by 3.4%, significant at 5%. The effect remains fairly constant in the two following years, and becomes stronger in the fourth year post moratorium (-4.3%). By the same token, we find a strongly

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<sup>4</sup> In the July 2008 SUSENAS survey, unusual consumption values were found in many districts (some examples are: Kabupaten Aceh Singkil, code 1102, Kabupaten Bogor, code 3201, and Kota Depok, code 3276). In these cases, the number of people living in households close to the poverty line (with per capita expenditure between 100% and 120% of the poverty line) resulted unusually low (or zero), which implied a break in the cumulative distribution function of per capita expenditure. These problems could be solved by imputation techniques, but we have not addressed this issue in the current version of the paper.

significant increase in poverty in the first three years after the introduction of the restriction to emigrate to Saudi Arabia. The average poverty rate increases by 2.6 percentage points in 2011, by 3.2 percentage points in 2012 and by 2.9 percentage points in 2013. Nonetheless, it is important to recognize that the significance of the coefficient in column [2] one year prior to the moratorium imposes caution in the interpretation of these estimates, as treated districts already appeared on a different trend one year prior to the moratorium.

Columns [3] to [6] in Table 6A report the estimated effect of the moratorium to Saudi Arabia on enrolment rates in primary and junior secondary education, separately for males and females. Results in columns [3] and [5] suggest that the moratorium to Saudi Arabia had no significant effect on the rate of enrolment in primary education. On the contrary, we find a significant (at 10%) increase in female enrolment in junior secondary school in the second year following the introduction of the restriction, and a more significant effect (at 5%) in the third and fourth year after the moratorium. The effects found on male enrolment in junior secondary education seem less significant than for females, and they decrease gradually over time. Since no differential pre-moratorium trends appear in columns [4] and [6] in Table 6A, we can attribute these results to the effect of the moratorium to Saudi Arabia.

Two potential channels could explain the increase in enrolment for pupils in junior secondary school following the moratorium. First, these results may reflect the importance of maternal presence for children's human capital investment: although the departure of female domestic workers to Saudi Arabia generates economic benefits for the origin households and communities, the departure of the mother can result in a loss of control over her children's schooling trajectories. This is consistent with the importance of spousal control over the intra-household allocation of resources documented in Antman (2011), as it suggests that mothers have bargaining power within the household. In the absence of any bargaining power, mothers' contribution to the household's welfare would work only through remittances, and the negative income shock induced by the



moratorium would be expected to result in a decrease in children's enrolment rate (e.g., children could be involved in labor activities rather than sent to school, as to compensate for the drop in remittances). Until recently, very few studies have focused on this issue (Antman 2013), mostly because female migration is less common in the countries the literature has focused on. Jampaklay (2006), instead, finds that, unlike paternal absence, the absence of the mother in the long run may impact negatively on children's education in Thailand. Cortes (2014) also concludes that the absence of the mother has an overall negative effect on children's education, further arguing that maternal absence is more harmful than the absence of the father. Our findings appear consistent with the evidence in these studies.

A second plausible explanation for the increase in enrolment rates in junior secondary education, and consistent with the fact that results are more pronounced for girls than for boys, relates to aspirations. While both authorities in Malaysia and Saudi Arabia require completion of primary school to be eligible to migrate as a domestic worker, alternative destination countries require completion of junior secondary school. These include Hong Kong, Singapore, Korea and Taiwan. Therefore, the positive effect of the moratorium on school enrolment for pupils in junior secondary school may be explained then by the rational decision of the households to invest further in their daughters' education, in order to enable them to migrate as domestic workers towards wealthy households in Hong Kong or Singapore. We cannot test this hypothesis at this stage, but we propose to undertake this important research venture as soon as new data will become available.

In the case of Malaysia (Table 6B), we also find a decrease in per capita expenditure by 2.8%, three and four years after the moratorium, but significant only at 10%. In general, the effects of the moratorium on the other outcomes appear non-significant. This could be explained on the one hand by the removal of the moratorium to Malaysia at the end of 2011. On the other hand, remittances from continuing undocumented flows, might have compensated for the drop in remittances driven by the decline in documented migrants.

## 6. Additional Estimates Using a Synthetic Controls Approach

In the last section of the paper, we assess the effects of the migration moratoria on local labor markets using a synthetic controls approach. This methodology proposes a data-driven approach to construct a suitable comparison group, and it has been famously applied in Abadie and Gardeazabal (2003) and Abadie et al. (2010). Since we analyze labor market outcomes at the province level, i.e., at a fairly aggregate level, and only 27 provinces existed in Indonesia during the entire time period of our analysis, we use this method to ensure comparability of our control provinces. By using a weighted combination of several unaffected provinces, this method ensures that treatment provinces are compared with a suitable control group. For this exercise, we use information from three sources of data. In order to calculate the degree of exposure across provinces to the migration moratoria, we use administrative data on migration flows for the pre-moratoria period (2008) collected by BNP2TKI. We calculate labor-market indicators for the period 2001-2014, using an annual province-level panel dataset created from the National Labor Force Survey (*Sakernas*), the main survey used to produce official indicators on employment and labor force participation by BPS-Statistics Indonesia. Finally, we also aggregate at the province level a rich array of information from *Podes* for the period 2000-2014, in order to build our “synthetic control” province.

For this analysis, we define our treatment and control provinces using administrative data from the first pre-moratoria year, 2008. We consider as provinces more likely to be intensely affected by the 2009 and 2011 moratoria, the provinces characterized by both high emigration rates and high incidence of migrants recruited in informal jobs (see for more details Appendix 2). Since a valid counterfactual does not exist for these provinces, the synthetic control province in our analysis was created as a weighted average of potential control provinces, and for each non-treatment province we used the information from *Podes* to match the “treatment” provinces over time along a number of characteristics. Further details on the variables used for the construction of our synthetic control province are provided in Appendix 3.

After building the synthetic control province, we investigated the consequences of moratoria on employment rate, labor-force participation rate and working hours, for men and women, women only, and low-educated adult women (women aged 18 to 54 with educational attainment lower than tertiary education). We considered these groups, since poorly educated adult women in “treatment” provinces were likely to be directly affected by the moratoria.

In order to verify the robustness of our conclusions, we also conducted a series of placebo tests following Abadie et al. (2010). For these placebo tests, we took a series of unaffected provinces as the treatment region, and we investigated labor-market effects in these provinces; since in reality these provinces were not affected by the moratoria, no effects should be visible on the labor-market indicators in this case. We conducted such placebo tests on the provinces of Bengkulu, Bangka Belitung, DKI Jakarta and Bali. The choice was driven by the fact that these are neighboring provinces to our true “treatment” provinces; however, in contrast to our true treatment group, informal migration is significantly less prevalent in placebo provinces. Our main results on employment rate, labor force participation rate, and hours worked are presented in Figures 6, 7 and 8, respectively, followed by the results of the placebo tests, shown in Figure 9.

The synthetic control analysis shows a decline in the employment rate in the order of 2 percentage points for the total active population in treatment provinces two years after the introduction of the first moratorium (Figure 6). The drop in employment rate reaches almost 3 percentage points for women, and 4 percentage points in the case of low-educated adult women. We also find a significant drop in labor force participation in the order of 2 percentage points for the total active population, and of 4 percentage points for both the full female sample and low-educated adult women (Figure 7). The results on hours worked show in general a slight increase in weekly working hours for all three groups considered (Figure 8). This could be explained by the fact that, conditionally on being employed, workers in treatment provinces need to work longer hours to

compensate for the loss of income from remittances. The magnitude of the effect found, however, is quite small, in the order of 1 extra hour of work per week for all the groups considered.

Results for these placebo provinces (Figure 9) look to be in stark contrast to the ones from our main analysis. This further validates the main conclusions of the synthetic control exercise, and suggests that the impact of moratoria on local labor markets documented so far is not merely driven by chance.

## **7. Conclusions and policy implications**

In this paper we study the consequences of a series of emigration policies introduced by the Indonesian government that restricted the migration of female domestic workers to traditional destination countries, such as Malaysia and Saudi Arabia. This is an unusual natural experiment, providing a rare opportunity to assess the effect of a migration restriction at the origin in the origin communities. In addition, since female domestic workers represented around 70% of all documented migrants before the introduction of moratoria, the natural experiment is likely to have important external validity.

Our general conclusion is that the migration moratoria implemented by the Indonesian government negatively affected the local labor markets at the origin. Using a variety of empirical approaches, our results suggest that the moratoria had a heterogeneous effect on international migration, and traditional origin communities of migrants seemingly experienced a more marked economic slowdown as a result of these restrictions. Local labor markets appeared unable to absorb the excess labor supply generated by the migration restrictions, and this ultimately resulted in a worsening of origin households' living conditions. Interestingly, we also find a positive effect of the moratorium to Saudi Arabia on enrolment in junior secondary school, arguably reflecting the importance of the maternal presence in the household for the schooling behavior of her children.

From the standpoint of migration policies, our results suggest that alternative policy options could be considered in order to make migrants more aware of, and better prepared for, the migration experience and the associated gains and losses. These policies could include more structured pre-departure trainings and information campaigns to raise migrants' awareness of overseas employment and living conditions, enforcement of compulsory insurance and protection schemes, and stricter monitoring of intermediaries' behavior. Finally, our results also call for the importance of elevating the policy debate on international migration beyond national borders, by encouraging cross-country bilateral agreements between sending and receiving countries that can maximize the options for safe and documented work for migrants overseas.

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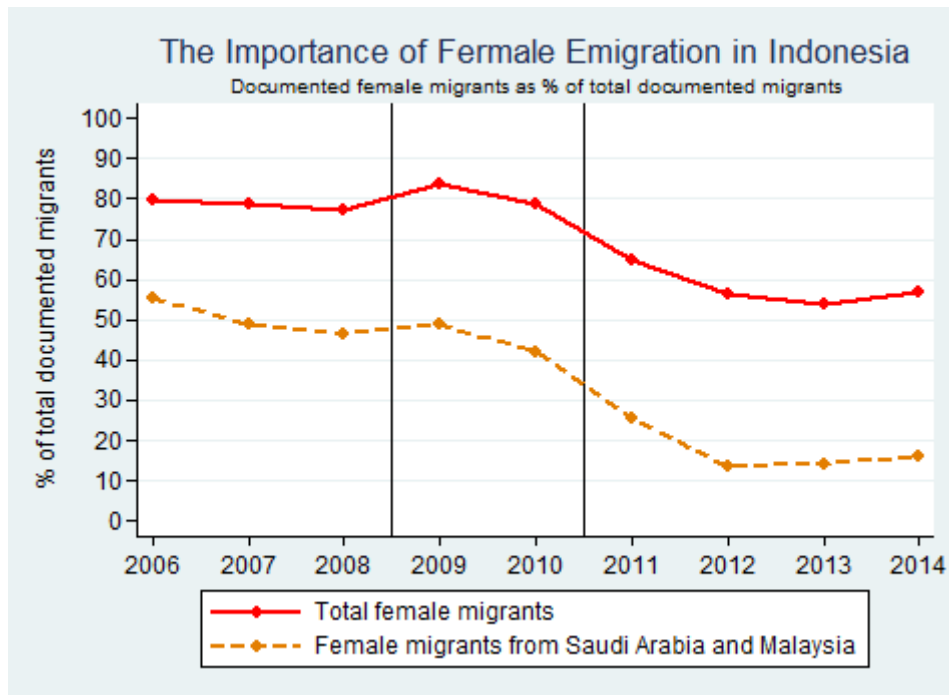
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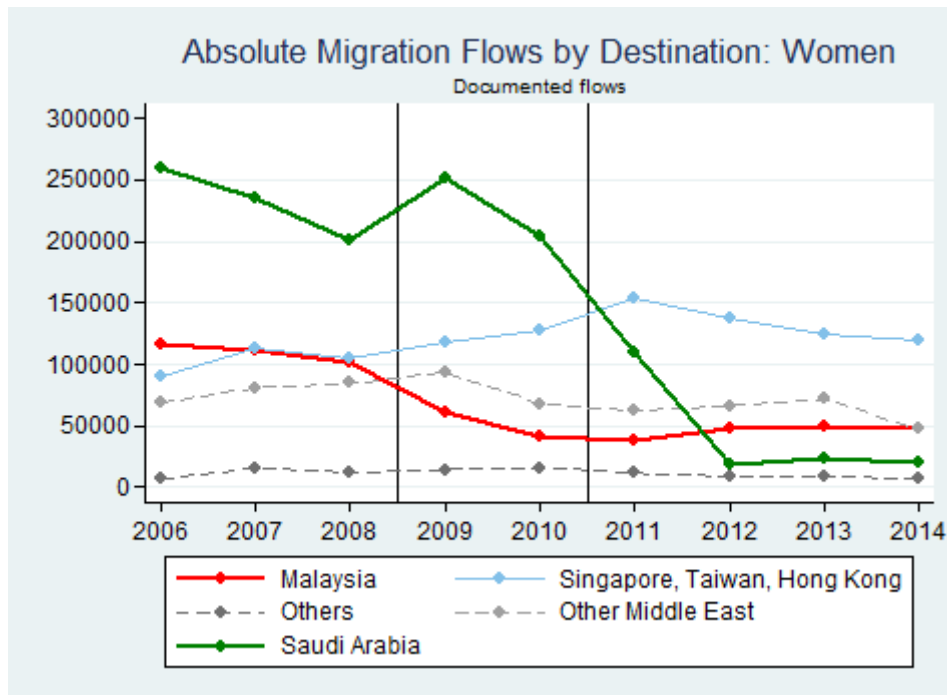
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**Figure 1. The importance of female migration in Indonesia**



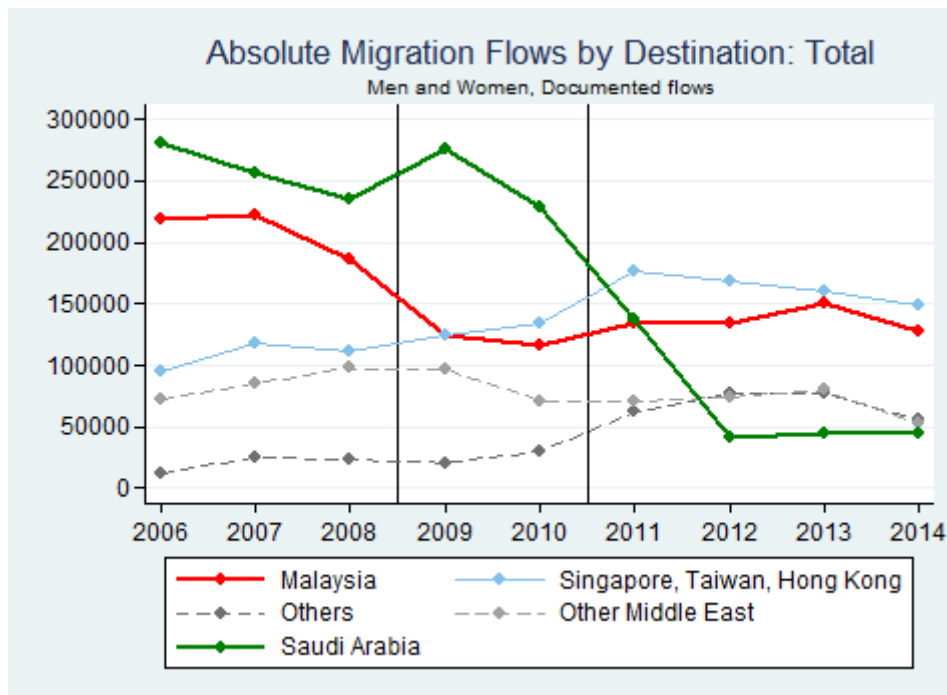
Source: BNP2TKI ([http://www.bnp2tki.go.id/stat\\_penempatan/indeks](http://www.bnp2tki.go.id/stat_penempatan/indeks)). Vertical line indicates the moratoria imposed to Malaysia (June 2009) and Saudi Arabia (June 2011).

**Figure 2A. Absolute flows of documented females migrants by destination country**



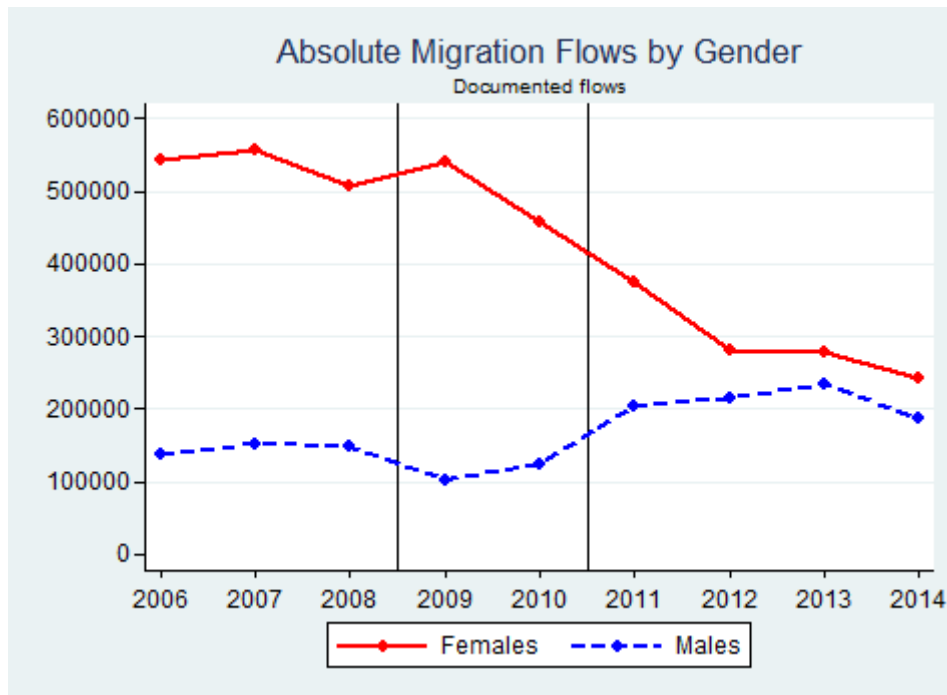
Source: BNP2TKI ([http://www.bn timer2tki.go.id/stat\\_penempatan/indeks](http://www.bn timer2tki.go.id/stat_penempatan/indeks)). Vertical line indicates the moratoria imposed to Malaysia (June 2009) and Saudi Arabia (June 2011). Other Middle East Countries are: United Arab Emirates, Kuwait, Qatar, Oman, Bahrein, Jordan. BNP2TKI data in this figure are end of the year data.

**Figure 2B. Absolute flows of total documented migrants by destination country: men and women**



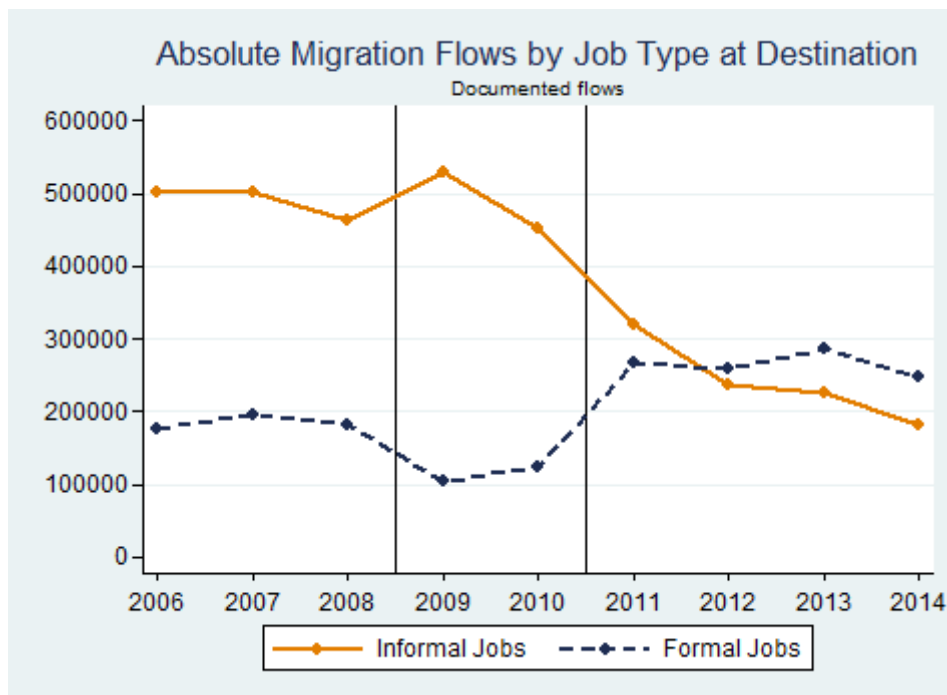
Source: BNP2TKI ([http://www.bn timer2tki.go.id/stat\\_penempatan/indeks](http://www.bn timer2tki.go.id/stat_penempatan/indeks)). Vertical line indicates the moratoria imposed to Malaysia (June 2009) and Saudi Arabia (June 2011). Other Middle East Countries are: United Arab Emirates, Kuwait, Qatar, Oman, Bahrein, Jordan. BNP2TKI data in this figure are end of the year data.

**Figure 3. Absolute flows of total documented migrants by gender**



Source: BNP2TKI ([http://www.bnptki.go.id/stat\\_penempatan/indeks](http://www.bnptki.go.id/stat_penempatan/indeks)). Vertical line indicates the moratoria imposed to Malaysia (June 2009) and Saudi Arabia (June 2011). Other Middle East Countries are: United Arab Emirates, Kuwait, Qatar, Oman, Bahrein, Jordan. BNP2TKI data in this figure are end of the year data.

**Figure 4. Absolute flows of total documented migrants by type of job placement in destination countries**



Source: BNP2TKI ([http://www.bnptki.go.id/stat\\_penempatan/indeks](http://www.bnptki.go.id/stat_penempatan/indeks)). Vertical line indicates the moratoria imposed to Malaysia (June 2009) and Saudi Arabia (June 2011). Other Middle East Countries are: United Arab Emirates, Kuwait, Qatar, Oman, Bahrein, Jordan. BNP2TKI data in this figure are end of the year data.

**Table 1A: Village Characteristics and Balancing Tests. Saudi A. Villages vs Others**

	<b>Saudi A. Villages</b>	<b>Other Villages</b>	<b>Treatment – Control Difference</b>
	[1]	[2]	[1]-[2]
Electricity Subscriber Households (log)	6.389	5.548	0.841*** (0.107)
Small Industries (log)	2.293	1.863	0.431*** (0.095)
Crime Occurrence	0.614	0.455	0.159*** (0.023)
% of Urban Villages ( <i>Kelurahan</i> )	0.064	0.131	-0.067*** (0.014)
% of Urban Villages in District	0.067	0.128	-0.060*** (0.013)
Number of Villages in District	299.290	285.781	13.509 (23.393)
Main Source of Income: Agriculture	0.875	0.851	0.024 (0.017)
Main Source of Income: Mining	0.002	0.002	-0.0004 (0.001)
Main Source of Income: Industry	0.030	0.021	0.010* (0.005)
Main Source of Income: Retail	0.050	0.055	-0.006 (0.007)
Main Source of Income: Other	0.043	0.071	-0.027*** (0.008)
Number of Kindergarten	1.269	1.235	0.034 (0.097)
Number of Primary Schools	3.389	2.573	0.817*** (0.159)
Number of High Schools	0.956	0.854	0.102* (0.062)
Number of Vocational Schools	0.399	0.394	0.006 (0.039)
Age Head Village	45.116	44.946	0.169 (0.338)
Male Head Village	0.972	0.971	0.001 (0.003)
Education Head Village	3.841	3.955	-0.114** (0.051)
Village Population	4762.215	3521.528	1240.687*** (270.455)
No. Total Migrant Workers (TKI)	46.617	16.923	29.694*** (6.405)
No. Male migrant workers (TKI)	7.550	8.950	-1.400 (1.604)
No. Female migrant workers (TKI)	39.067	7.973	31.094*** (5.615)
Number of Villages	6745	36748	

**Table 1B: Village Characteristics and Balancing Tests. Malaysia Villages vs Others**

	Malaysia Villages	Other Villages	Treatment – Control Difference
	[1]	[2]	[1]-[2]
Electricity Subscriber Households (log)	5.882	5.554	0.329*** (0.094)
Small Industries (log)	2.179	1.778	0.402*** (0.071)
Crime Occurrence	0.510	0.462	0.048** (0.019)
% of Urban Villages ( <i>Kelurahan</i> )	0.098	0.135	-0.037*** (0.012)
% of Urban Villages in District	0.102	0.128	-0.026** (0.012)
Number of Villages in District	300.581	280.154	20.427 (14.464)
Main Source of Income: Agriculture	0.885	0.837	0.048*** (0.013)
Main Source of Income: Mining	0.001	0.003	-0.002*** (0.001)
Main Source of Income: Industry	0.019	0.024	-0.006* (0.003)
Main Source of Income: Retail	0.044	0.061	-0.016*** (0.005)
Main Source of Income: Other	0.052	0.075	-0.024*** (0.007)
Number of Kindergarten	1.451	1.113	0.338*** (0.077)
Number of Primary Schools	2.810	2.632	0.179 (0.127)
Number of High Schools	0.869	0.871	-0.001 (0.060)
Number of Vocational Schools	0.361	0.415	-0.054 (0.039)
Age Head Village	44.869	45.036	-0.167 (0.206)
Male Head Village	0.970	0.971	-0.001 (0.002)
Education Head Village	4.041	3.874	0.167*** (0.039)
Village Population	3705.428	3719.108	-13.680 (242.008)
No. Total Migrant Workers (TKI)	32.100	15.102	16.998*** (4.699)
No. Male migrant workers (TKI)	18.213	2.971	15.242*** (3.232)
No. Female migrant workers (TKI)	13.887	12.131	1.755 (2.408)
Number of Villages	16442	27051	



**Table 2: OLS Estimates of Impact of Moratoria on Number of Migrants**

	Dependent Variable: Number of migrants (Log)			Dependent Variable: Number of migrants (Log)		
	OLS [1]	OLS [2]	OLS [3]	OLS [1]	OLS [2]	OLS [3]
Moratorium to Malaysia	-0.050** (-2.31)	-0.045** (-2.12)	-0.368*** (-23.21)			
Malaysia Village*Moratorium	-0.417*** (-16.37)	-0.416*** (-16.42)	-0.489*** (-16.54)			
Moratorium to Saudi Arabia				-0.096*** (-6.46)	-0.094*** (-6.34)	-0.191*** (-13.87)
Saudi Village*Moratorium				-0.260*** (-9.55)	-0.260*** (-9.63)	-0.366*** (-11.61)
Village Controls	No	Yes	Yes	No	Yes	Yes
District-specific time trends	No	No	Yes	No	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	176892	176892	176892	176892	176892	176892
Number of Villages	45649	45649	45649	45649	45649	45649
Number of Districts	274	274	274	274	274	274

Notes: \* indicates 10% significance, \*\* indicates 5% significance and \*\*\* indicates 1% significance. Robust standard errors (clustered at the district level) are reported in parentheses. Village control variables included are whether the village is *kelurahan* (located in an urban area) vs. *desa*, the fraction of villages in the district classified as *kelurahan*, the number of villages in the district, and a series of mutually exclusive dummies indicating the sector which constitutes the major source of income for the village, entered together with the quadratic of age of the village head, the gender of the village head and the level of education of the village head. With “district”, here we mean *kabupaten/kota*, known in Indonesia as “regencies”/“cities”, the main administrative subdivision of provinces. From the administrative standpoint, *Kabupaten/kota* include various *kecamatan* (“sub-districts”), which in turn are subdivided in villages (*kelurahan/desa*).

**Table 3A. Effect of moratorium to Saudi Arabia on electricity subscriber households**

<b>Dependent variable:</b>	All	All	Rural	Rural
<b>Log of electricity subscribers</b>	[1]	[2]	[3]	[4]
Saudi village*Dummy year=2002 [9 years pre-moratorium]	-0.00466 (-0.16)	-0.00790 (-0.27)	-0.0160 (-0.52)	-0.0200 (-0.66)
Saudi village*Dummy year=2005 [6 years pre-moratorium]	-0.0367 (-0.96)	-0.0448 (-1.18)	-0.0576 (-1.42)	-0.0653 (-1.61)
Saudi village*Dummy year=2008 [3 years pre-moratorium]	-0.0239 (-0.66)	-0.0391 (-1.14)	-0.0536 (-1.39)	-0.0685* (-1.88)
<b>Saudi village*Dummy year=2011</b> [moratorium is introduced]	<b>-0.133***</b> (-2.72)	<b>-0.150***</b> (-3.22)	<b>-0.179***</b> (-3.39)	<b>-0.197***</b> (-3.93)
<b>Saudi village*Dummy year=2014</b> [3 years post-moratorium]	<b>-0.151***</b> (-3.04)	<b>-0.178***</b> (-3.76)	<b>-0.199***</b> (-3.67)	<b>-0.230***</b> (-4.47)
Village fixed effects	Yes	Yes	Yes	Yes
Survey fixed effects	Yes	Yes	Yes	Yes
Main sources of income (village)	Yes	Yes	Yes	Yes
Number of schools by type (village)	No	Yes	No	Yes
R <sup>2</sup>	0.309	0.320	0.319	0.330
N	264219	264168	233216	233166

*t* statistics in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01

**Table 3B. Effect of moratorium to Malaysia on electricity subscriber households**

<b>Dependent variable:</b>	All	All	Rural	Rural
<b>Log of electricity subscribers</b>	[1]	[2]	[3]	[4]
Malaysia village*Dummy year=2002 [7 years pre-moratorium]	-0.0444 (-1.61)	-0.0470* (-1.71)	-0.0526* (-1.77)	-0.0525* (-1.78)
Malaysia village*Dummy year=2005 [4 years pre-moratorium]	-0.0140 (-0.41)	-0.0181 (-0.52)	-0.0201 (-0.53)	-0.0213 (-0.57)
Malaysia village*Dummy year=2008 [1 years pre-moratorium]	-0.0103 (-0.29)	-0.0120 (-0.34)	-0.0267 (-0.69)	-0.0262 (-0.68)
<b>Malaysia village*Dummy year=2011</b> [2 years after moratorium was introduced]	<b>-0.0820*</b> (-1.92)	<b>-0.0798*</b> (-1.88)	<b>-0.109**</b> (-2.29)	<b>-0.105**</b> (-2.23)
<b>Malaysia village*Dummy year=2014</b> [5 years post-moratorium]	<b>-0.0935**</b> (-2.06)	<b>-0.0904**</b> (-2.01)	<b>-0.119**</b> (-2.37)	<b>-0.113**</b> (-2.28)
Village fixed effects	Yes	Yes	Yes	Yes
Survey fixed effects	Yes	Yes	Yes	Yes
Main sources of income (village)	Yes	Yes	Yes	Yes
Number of schools by type (village)	No	Yes	No	Yes
R <sup>2</sup>	0.309	0.319	0.318	0.329
N	264219	264168	233216	233166

*t* statistics in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01

**Table 4A. Effect of moratorium to Saudi Arabia on the number of small firms**

<b>Dependent variable:</b>	All	All	Rural	Rural
<b>Log of number of small firms</b>	[1]	[2]	[3]	[4]
Saudi village*Dummy year=2002 [9 years pre-moratorium]	-0.0826 (-1.31)	-0.0776 (-1.23)	-0.102 (-1.54)	-0.0961 (-1.45)
Saudi village*Dummy year=2005 [6 years pre-moratorium]	0.0402 (0.69)	0.0361 (0.62)	0.0260 (0.43)	0.0227 (0.37)
Saudi village*Dummy year=2008 [3 years pre-moratorium]	0.0695 (1.19)	0.0279 (0.50)	0.0659 (1.06)	0.0235 (0.40)
<b>Saudi village*Dummy year=2011</b> [moratorium is introduced]	<b>0.109*</b> (1.86)	<b>0.0590</b> (1.06)	<b>0.117*</b> (1.88)	<b>0.0648</b> (1.10)
<b>Saudi village*Dummy year=2014</b> [3 years post-moratorium]	<b>0.128*</b> (1.96)	<b>0.0735</b> (1.22)	<b>0.147**</b> (2.12)	<b>0.0886</b> (1.38)
Village fixed effects	Yes	Yes	Yes	Yes
Survey fixed effects	Yes	Yes	Yes	Yes
Main sources of income (village)	Yes	Yes	Yes	Yes
Number of schools by type (village)	No	Yes	No	Yes
R <sup>2</sup>	0.064	0.069	0.061	0.066
N	264164	264164	233162	233162

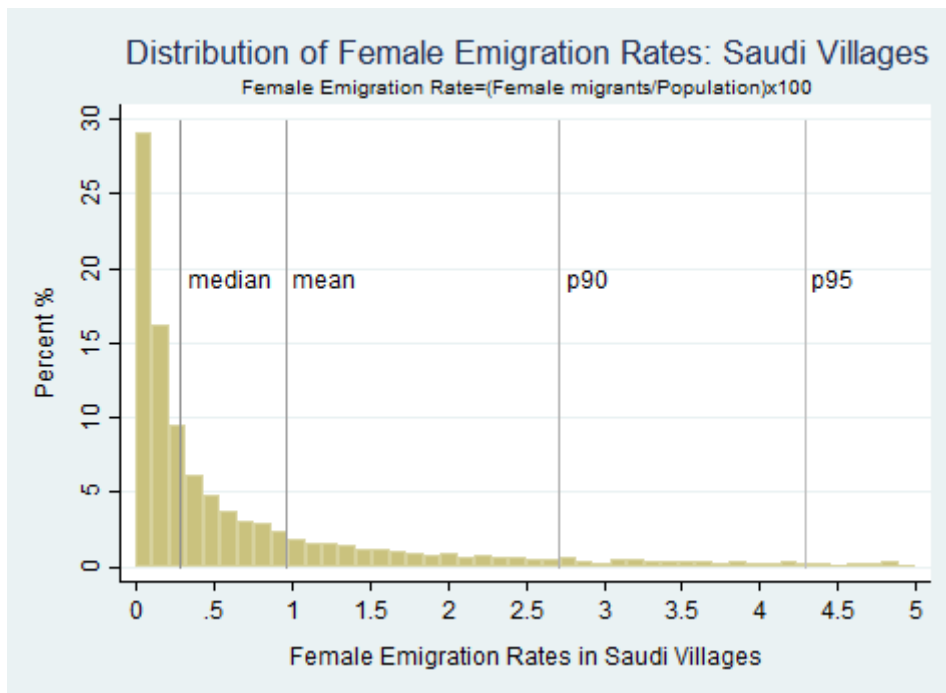
*t* statistics in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01

**Table 4B. Effect of moratorium to Malaysia on the number of small firms**

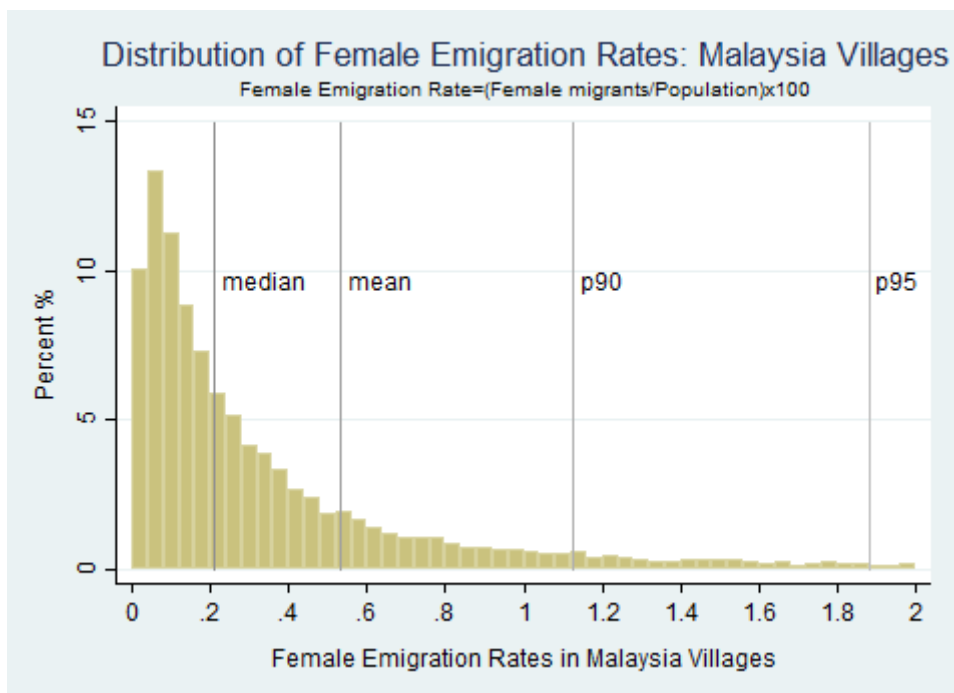
<b>Dependent variable:</b>	All	All	Rural	Rural
<b>Log of number of small firms</b>	[1]	[2]	[3]	[4]
Malaysia village*Dummy year=2002 [7 years pre-moratorium]	-0.0179 (-0.44)	-0.0191 (-0.47)	-0.0238 (-0.55)	-0.0241 (-0.56)
Malaysia village*Dummy year=2005 [4 years pre-moratorium]	0.0266 (0.78)	0.0258 (0.76)	0.0286 (0.80)	0.0292 (0.82)
Malaysia village*Dummy year=2008 [1 years pre-moratorium]	0.0496 (1.14)	0.0554 (1.30)	0.0598 (1.28)	0.0682 (1.50)
<b>Malaysia village*Dummy year=2011</b> [2 years after moratorium was introduced]	<b>0.0294</b> (0.72)	<b>0.0373</b> (0.94)	<b>0.0523</b> (1.20)	<b>0.0624</b> (1.47)
<b>Malaysia village*Dummy year=2014</b> [5 years post-moratorium]	<b>0.0242</b> (0.53)	<b>0.0299</b> (0.66)	<b>0.0417</b> (0.86)	<b>0.0467</b> (0.98)
Village fixed effects	Yes	Yes	Yes	Yes
Survey fixed effects	Yes	Yes	Yes	Yes
Main sources of income (village)	Yes	Yes	Yes	Yes
Number of schools by type (village)	No	Yes	No	Yes
R <sup>2</sup>	0.064	0.069	0.061	0.066
N	264164	264164	233162	233162

*t* statistics in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01

**Figure 5A. Frequency distribution of female emigration rates in Saudi villages.**



**Figure 5B. Frequency distribution of female emigration rates in Malaysia villages.**



**Table 5A. Diff-in-diff estimates of Moratoria to Saudi Arabia interacted with continuous treatment (Migration Intensity).**

Dependent Variables	Electricity subscriber households (log)		Number of small industries (log)	
	All	Rural	All	Rural
	[1]	[2]	[3]	[4]
Saudi village*Dummy year=2002 [9 years pre-moratorium]	-0.00234 (-0.08)	-0.0130 (-0.41)	-0.0502 (-0.87)	-0.0711 (-1.17)
Saudi village*Dummy year=2005 [6 years pre-moratorium]	-0.0376 (-0.96)	-0.0572 (-1.37)	0.0734 (1.29)	0.0599 (1.00)
Saudi village*Dummy year=2008 [3 years pre-moratorium]	-0.0583 (-1.58)	-0.0854** (-2.17)	-0.00147 (-0.03)	-0.00671 (-0.12)
Saudi village*Dummy year=2002* Migration Intensity	-0.0108 (-0.59)	-0.0105 (-0.57)	-0.0155 (-0.47)	-0.0109 (-0.32)
Saudi village*Dummy year=2005* Migration Intensity	-0.0173 (-0.92)	-0.0153 (-0.81)	-0.0181 (-0.60)	-0.0188 (-0.62)
Saudi village*Dummy year=2008* Migration Intensity	-0.0123 (-0.71)	-0.00821 (-0.46)	0.00582 (0.20)	0.00324 (0.11)
<b>Saudi village*Dummy year=2011</b> [moratorium is introduced]	<b>-0.156***</b> (-3.36)	<b>-0.204***</b> (-4.06)	<b>0.0945*</b> (1.75)	<b>0.104*</b> (1.84)
<b>Saudi village*Dummy year=2014</b> [3 years post-moratorium]	<b>-0.182***</b> (-3.86)	<b>-0.237***</b> (-4.59)	<b>0.0810</b> (1.47)	<b>0.0983*</b> (1.66)
<b>Saudi village*Dummy year=2011*</b> <b>Migration Intensity</b>	<b>0.00755</b> (0.32)	<b>0.0162</b> (0.68)	<b>-0.0408</b> (-1.39)	<b>-0.0477*</b> (-1.66)
<b>Saudi village*Dummy year=2014*</b> <b>Migration Intensity</b>	<b>0.00721</b> (0.31)	<b>0.0168</b> (0.71)	<b>-0.0245</b> (-0.79)	<b>-0.0306</b> (-1.01)
Village fixed effects	Yes	Yes	Yes	Yes
Survey fixed effects	Yes	Yes	Yes	Yes
Main sources of income (village)	Yes	Yes	Yes	Yes
Number of schools by type (village)	Yes	Yes	Yes	Yes
R <sup>2</sup>	0.321	0.331	0.069	0.066
N	261388	230661	261384	230657

*t* statistics in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01

**Table 5B. Diff-in-diff estimates of moratoria to Malaysia interacted with continuous treatment (Migration Intensity).**

Dependent Variables	Electricity subscriber households (log)		Number of small industries (log)	
	All	Rural	All	Rural
	[1]	[2]	[3]	[4]
Malaysia village*Dummy year=2002 [7 years pre-moratorium]	-0.0587** (-2.04)	-0.0674** (-2.16)	-0.0305 (-0.76)	-0.0377 (-0.88)
Malaysia village*Dummy year=2005 [4 years pre-moratorium]	-0.0369 (-1.02)	-0.0432 (-1.09)	0.0332 (0.99)	0.0357 (1.00)
Malaysia village*Dummy year=2008 [1 years pre-moratorium]	-0.0228 (-0.63)	-0.0398 (-1.00)	0.0567 (1.29)	0.0700 (1.50)
Malaysia village*Dummy year=2002* Migration Intensity	0.0283 (1.35)	0.0329 (1.54)	0.0309 (1.03)	0.0338 (1.12)
Malaysia village*Dummy year=2005* Migration Intensity	0.0449** (1.97)	0.0492** (2.12)	-0.0148 (-0.60)	-0.0123 (-0.50)
Malaysia village*Dummy year=2008* Migration Intensity	0.0322 (1.51)	0.0382* (1.77)	-0.00994 (-0.35)	-0.00962 (-0.34)
<b>Malaysia village*Dummy year=2011</b> [2 years after moratorium was introduced]	<b>-0.104**</b> (-2.38)	<b>-0.134***</b> (-2.77)	<b>0.0334</b> (0.84)	<b>0.0589</b> (1.39)
<b>Malaysia village*Dummy year=2014</b> [5 years post-moratorium]	<b>-0.119**</b> (-2.54)	<b>-0.148***</b> (-2.85)	<b>0.0262</b> (0.59)	<b>0.0432</b> (0.91)
<b>Malaysia village*Dummy year=2011*</b> <b>Migration Intensity</b>	<b>0.0651**</b> (2.38)	<b>0.0747***</b> (2.68)	<b>0.0120</b> (0.43)	<b>0.0109</b> (0.40)
<b>Malaysia village*Dummy year=2014*</b> <b>Migration Intensity</b>	<b>0.0760**</b> (2.59)	<b>0.0854***</b> (2.86)	<b>-0.00101</b> (-0.04)	<b>-0.00178</b> (-0.07)
Village fixed effects	Yes	Yes	Yes	Yes
Survey fixed effects	Yes	Yes	Yes	Yes
Main sources of income (village)	Yes	Yes	Yes	Yes
Number of schools by type (village)	Yes	Yes	Yes	Yes
R <sup>2</sup>	0.320	0.330	0.069	0.066
N	262410	231559	262406	231555

*t* statistics in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01

**Table 6A. Effects of moratorium to Saudi Arabia on consumption, poverty and enrolment rates**

	Log Per capita expenditure	Poverty status	Primary Enrolment (Males)	Junior Secondary Enrolment (Males)	Primary Enrolment (Females)	Junior Secondary Enrolment (Females)
	[1]	[2]	[3]	[4]	[5]	[6]
Saudi village*Dummy year=2005 [6 years pre-moratorium]	0.004 (0.436)	0.002 (0.511)	-0.998* (-1.817)	1.383 (1.328)	-0.453 (-0.860)	-1.816* (-1.658)
Saudi village*Dummy year=2006 [5 years pre-moratorium]	-0.011 (-1.164)	0.004 (1.043)	-1.108** (-2.156)	0.597 (0.514)	-0.317 (-0.678)	-1.288 (-1.029)
Saudi village*Dummy year=2007 [4 years pre-moratorium]	0.003 (0.279)	0.003 (0.774)	-0.429 (-0.841)	0.330 (0.277)	0.232 (0.499)	-0.092 (-0.077)
Saudi village*Dummy year=2008 [3 years pre-moratorium]	N/A N/A	N/A N/A	0.255 (0.400)	-0.014 (-0.008)	0.238 (0.392)	0.096 (0.066)
Saudi village*Dummy year=2009 [2 years pre-moratorium]	-0.018 (-1.645)	0.008 (1.500)	-1.302* (-1.941)	1.215 (0.784)	-1.594*** (-2.819)	1.450 (0.912)
Saudi village*Dummy year=2010 [1 year pre-moratorium]	-0.021* (-1.678)	0.019*** (3.021)	-0.858* (-1.668)	-1.676 (-1.275)	-1.344** (-2.510)	0.691 (0.488)
<b>Saudi village*Dummy year=2011</b> [moratorium is introduced]	<b>-0.034**</b> (-1.990)	<b>0.026***</b> (2.806)	<b>-0.299</b> (-0.551)	<b>4.133***</b> (2.864)	<b>-0.238</b> (-0.404)	<b>1.951</b> (1.285)
<b>Saudi village*Dummy year=2012</b> [1 year post-moratorium]	<b>-0.032*</b> (-1.728)	<b>0.032***</b> (2.858)	<b>-0.226</b> (-0.377)	<b>3.004*</b> (1.826)	<b>-0.504</b> (-1.098)	<b>3.173*</b> (1.925)
<b>Saudi village*Dummy year=2013</b> [2 years post-moratorium]	<b>-0.030*</b> (-1.654)	<b>0.029***</b> (2.797)	<b>0.160</b> (0.273)	<b>2.888*</b> (1.884)	<b>-0.071</b> (-0.137)	<b>3.666**</b> (2.524)
<b>Saudi village*Dummy year=2014</b> [3 years post-moratorium]	<b>-0.043**</b> (-2.159)	<b>0.015</b> (1.565)	<b>-0.538</b> (-0.897)	<b>2.045</b> (1.454)	<b>-1.092**</b> (-2.255)	<b>2.865**</b> (1.983)
Survey fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R <sup>2</sup>	0.623	0.121	0.019	0.042	0.019	0.041
N	10909600	10909600	4177345	2164609	3916086	2035560

t statistics in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. Consumption and poverty variables from 2008 not included in the estimations for data quality issues mentioned in the text.

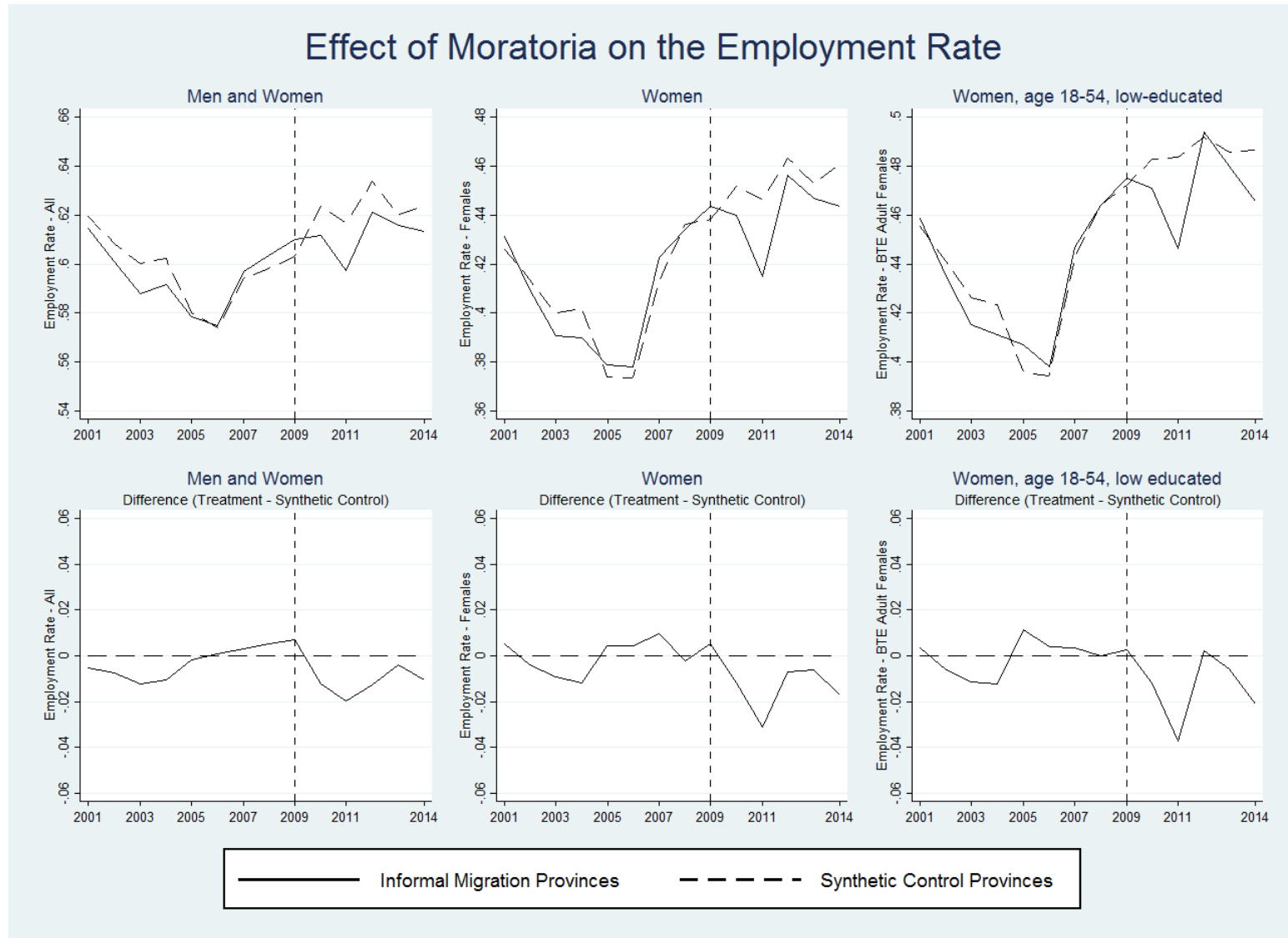
**Table 6B. Effects of moratorium to Malaysia on consumption, poverty and enrolment rates**

	Log Per capita expenditure	Poverty status	Primary Enrolment (Males)	Junior Secondary Enrolment (Males)	Primary Enrolment (Females)	Junior Secondary Enrolment (Females)
	[1]	[2]	[3]	[4]	[5]	[6]
Malaysia village*Dummy year=2003 [6 years pre-moratorium]	-0.009 (-0.975)	0.006 (1.321)	-0.625 (-1.233)	-0.286 (-0.289)	-0.773 (-1.522)	-1.694 (-1.590)
Malaysia village*Dummy year=2004 [5 years pre-moratorium]	-0.027** (-2.342)	0.008 (1.453)	-0.745 (-1.401)	0.359 (0.310)	-0.680 (-1.285)	0.535 (0.467)
Malaysia village*Dummy year=2005 [4 years pre-moratorium]	-0.004 (-0.365)	-0.003 (-0.539)	0.209 (0.386)	-1.634 (-1.432)	0.307 (0.549)	-0.278 (-0.235)
Malaysia village*Dummy year=2006 [3 years pre-moratorium]	0.008 (0.721)	-0.006 (-1.021)	-0.092 (-0.166)	-1.156 (-0.932)	0.209 (0.384)	0.699 (0.560)
Malaysia village*Dummy year=2007 [2 year pre-moratorium]	-0.015 (-1.247)	-0.004 (-0.702)	-0.812 (-1.436)	0.733 (0.592)	-0.632 (-1.128)	0.232 (0.194)
Malaysia village*Dummy year=2008 [1 year pre-moratorium]	N/A N/A	N/A N/A	-1.140* (-1.760)	0.624 (0.415)	-0.481 (-0.743)	-0.912 (-0.642)
<b>Malaysia village*Dummy year=2009</b> [moratorium is introduced]	<b>-0.005</b> (-0.463)	<b>-0.005</b> (-0.770)	<b>0.093</b> (0.137)	<b>0.054</b> (0.037)	<b>0.992</b> (1.629)	<b>-0.286</b> (-0.196)
<b>Malaysia village*Dummy year=2010</b> [1 year post-moratorium]	<b>-0.012</b> (-0.880)	<b>-0.010</b> (-1.489)	<b>-0.188</b> (-0.320)	<b>2.151</b> (1.536)	<b>0.319</b> (0.548)	<b>-0.317</b> (-0.227)
<b>Malaysia village*Dummy year=2011</b> [2 year post-moratorium]	<b>-0.025</b> (-1.572)	<b>0.000</b> (0.038)	<b>0.052</b> (0.081)	<b>-1.697</b> (-1.106)	<b>0.104</b> (0.169)	<b>-1.738</b> (-1.161)
<b>Malaysia village*Dummy year=2012</b> [3 year post-moratorium]	<b>-0.028*</b> (-1.658)	<b>-0.003</b> (-0.342)	<b>0.546</b> (0.861)	<b>-1.086</b> (-0.691)	<b>0.620</b> (1.074)	<b>-1.046</b> (-0.687)
<b>Malaysia village*Dummy year=2013</b> [4 year post-moratorium]	<b>-0.028*</b> (-1.698)	<b>-0.005</b> (-0.519)	<b>0.448</b> (0.713)	<b>-0.508</b> (-0.341)	<b>0.581</b> (0.978)	<b>-0.691</b> (-0.464)
<b>Malaysia village*Dummy year=2014</b> [5 year post-moratorium]	<b>-0.019</b> (-1.103)	<b>-0.017*</b> (-1.862)	<b>0.035</b> (0.054)	<b>-1.367</b> (-0.966)	<b>0.259</b> (0.439)	<b>-1.223</b> (-0.820)
Survey fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R <sup>2</sup>	0.623	0.121	0.019	0.042	0.019	0.041
N	10909600	10909600	4177345	2164609	3916086	2035560

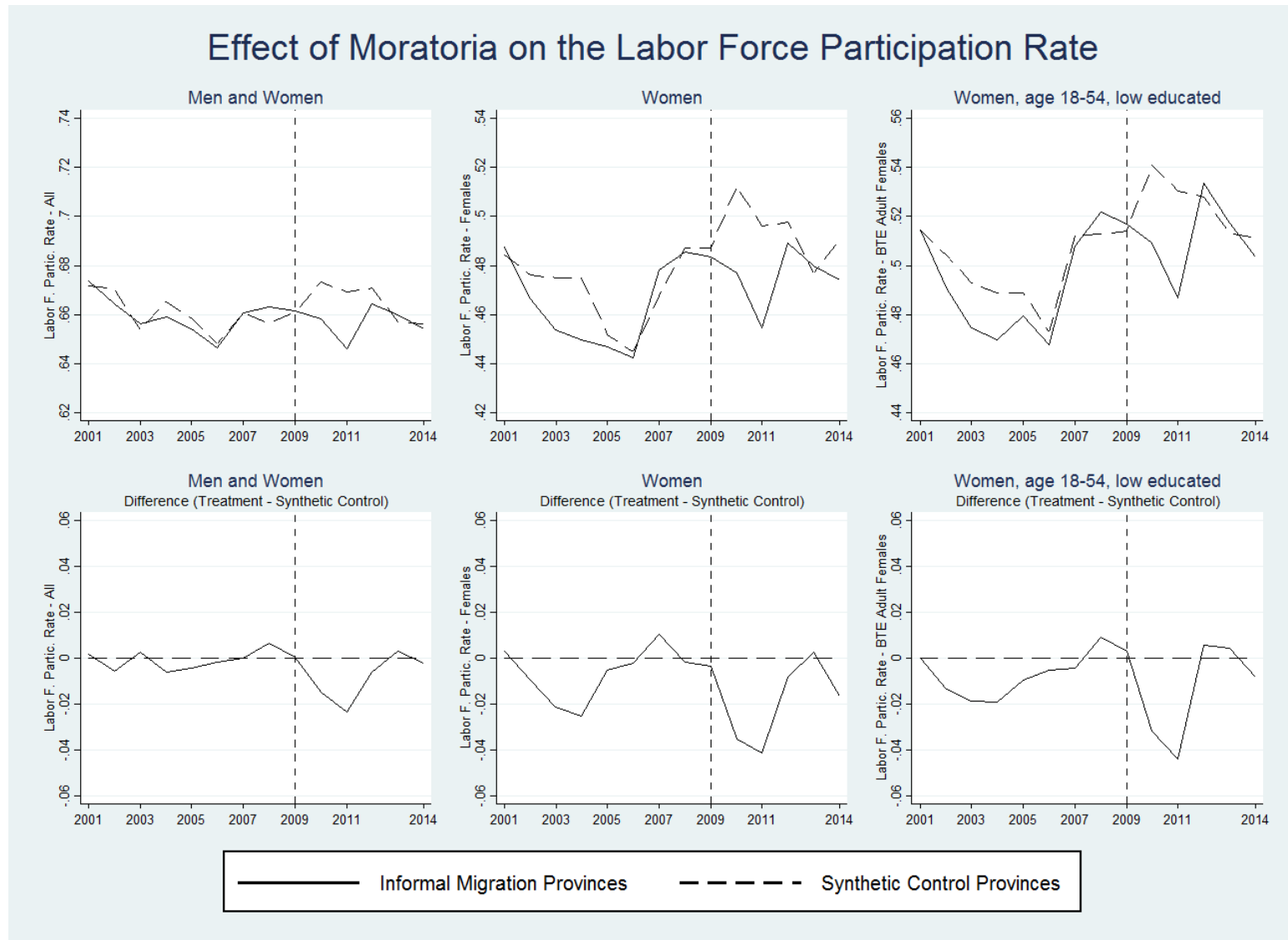
*t* statistics in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. Consumption and poverty variables from 2008 not included in the estimations for data quality issues mentioned in the text.



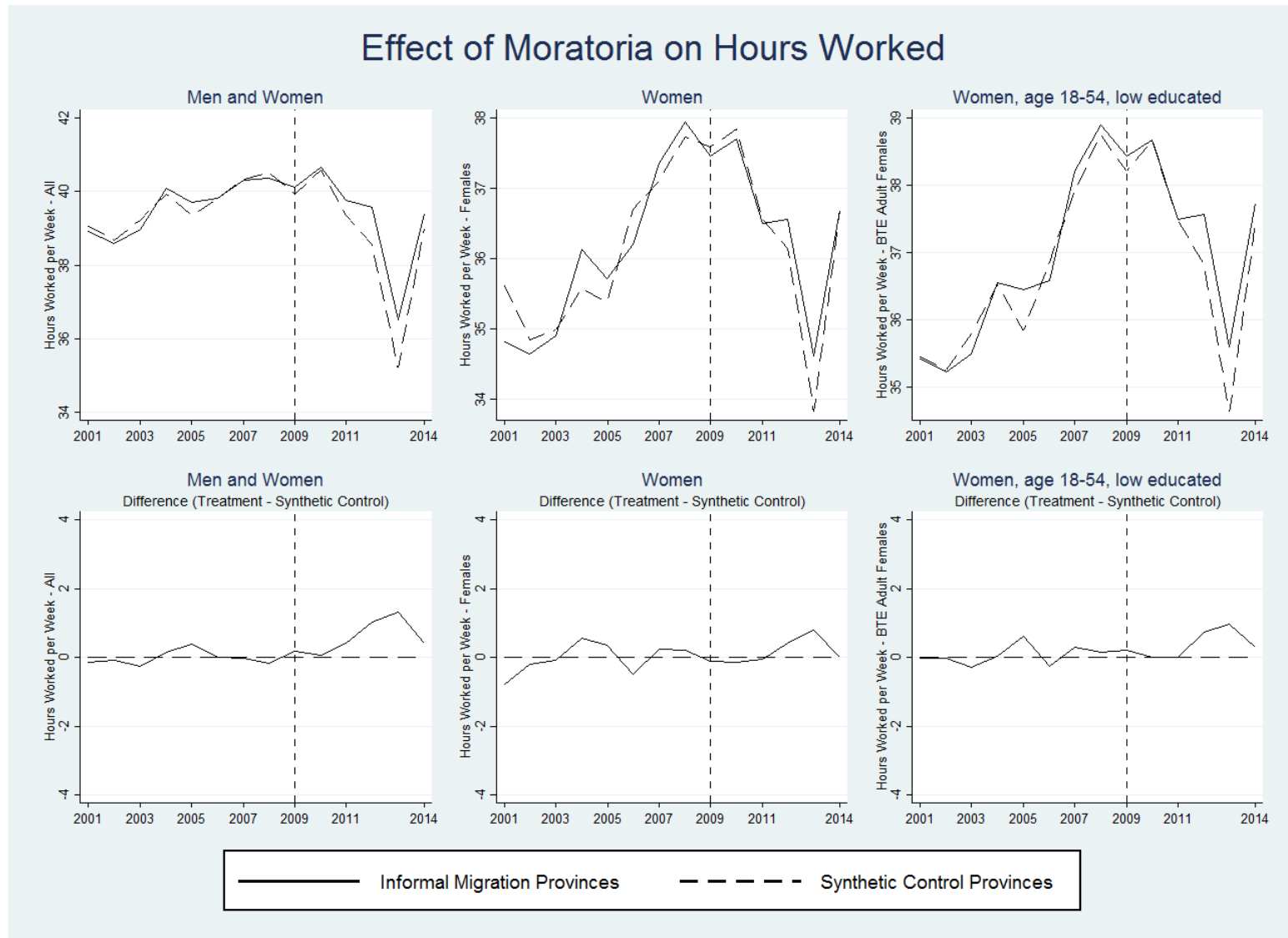
**Figure 6. Effect of Moratoria on the Employment Rate.**



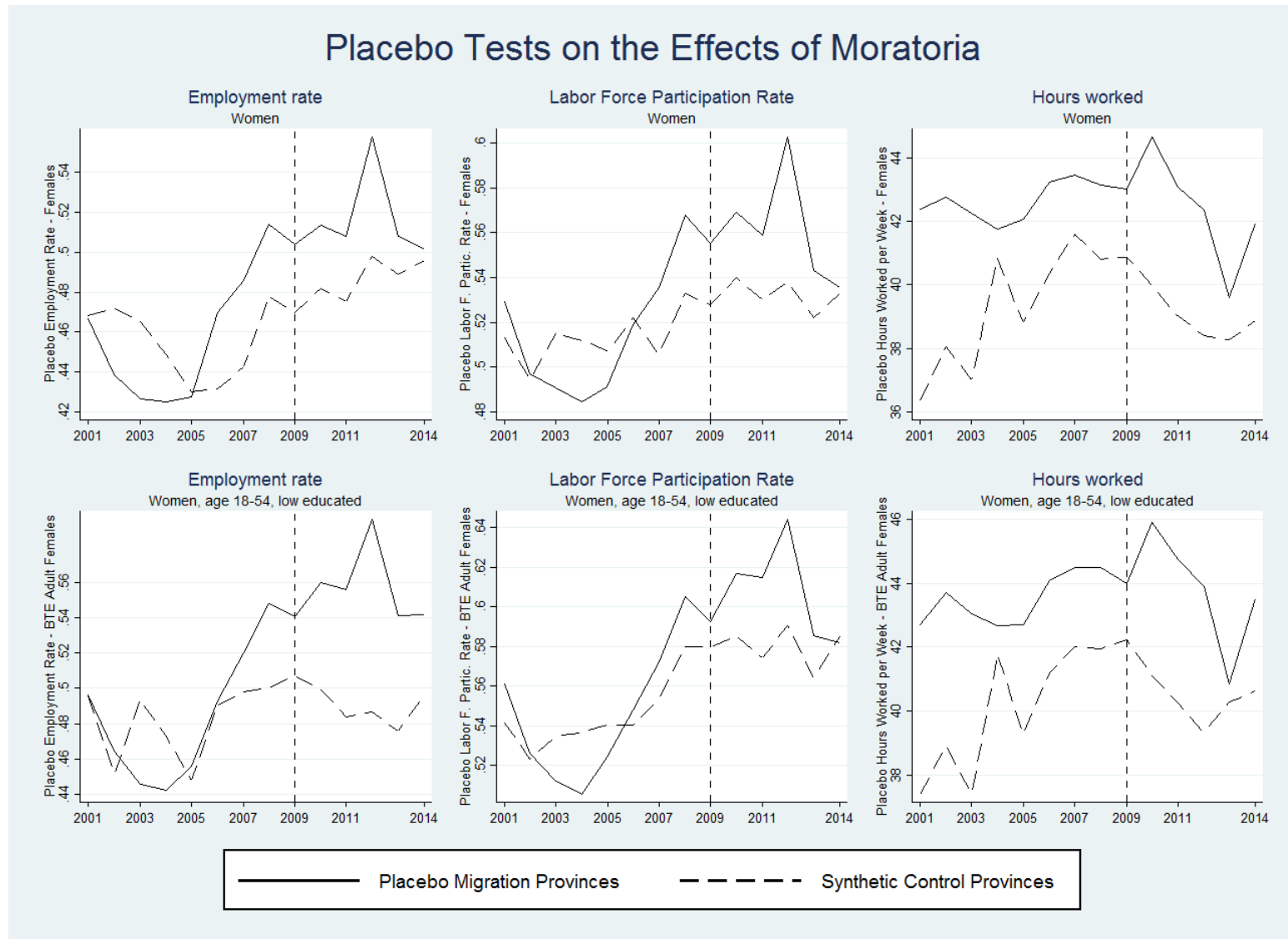
**Figure 7. Effect of Moratoria on the Labor Force Participation Rate.**



**Figure 8. Effect of Moratoria on the Hours Worked.**



**Figure 9. Placebo tests on the Effects of Moratoria.**



## **Appendix 1. KTKLN Card Application: required steps for perspective migrants**

1. Obtain information about the documentation necessary to emigrate for work from the local Office of Manpower (Disnaker) or the local Office of Placement Services and Protection of Indonesian Migrant Workers (BP3TKI)
2. Prepare ID card, birth certificate, and school diploma.
3. Prepare a permission letter from spouse/parent/ guardian, verified by the village head.
4. Fill in a Job Seeker Registration Card issued by Disnaker.
5. Register as a prospective migrant worker at Disnaker office.
6. Attend the socialization meeting conducted by Disnaker to inform perspective migrants on available job vacancies abroad.
7. Attend interests and skills selection tests conducted by Disnaker and PPTKIS (in case the profile of the prospective migrant fits the criteria of the job vacancy).
8. Sign the Placement Agreement with PPTKIS (verified by Disnaker) if the selection test is passed.
9. Reside in temporary accommodations/shelters owned by PPTKIS before departure (for prospective migrant workers selected for informal jobs).
10. Attend trainings, and receive a certificate of attendance.
11. Attend competency test conducted by Professional Certifying Agency, and obtain a skill certificate.
12. Undertake health test
13. Undertake a psychological test
14. Apply for passport
15. Apply for work permit
16. Apply for visa
17. Apply for employment insurance, and obtain an insurance card
18. Contribute to the Labor Development Fund.
19. Attend Pre-Departure Briefing (PAP)
20. Sign a job contract with perspective employer or agency.
21. The migrant worker ID card (KTKLN) is issued by BNP2TKI
22. Depart to destination country

*Source:* Pocket Book for Prospective Migrant Worker: Working Abroad Legally and Safely, 2011, developed by IOM, United States Government Office to Monitor and Combat Trafficking in Persons (G/TIP), BNP2TKI, and the Ministry of Manpower.

## Appendix 2. Identification of “treatment” provinces for the synthetic control analysis.

The definition of informal migrants in the BNP2TKI database is made on the basis of the job undertaken overseas. Since domestic workers are most likely to fall into this category, the label “informal migrant” is therefore an imperfect but plausible proxy for domestic workers. Province-level data prior to 2008 were not available. The ranking of Indonesian provinces according to both the Informal/Formal migrants ratio and the Emigration rate is presented in Table 7 below. We considered as “treatment” provinces those ranked in the top 20% according to both indicators. This condition restricts the choice set to Lampung, West Java and East Java, which are defined then as “treatment provinces” in the synthetic controls analysis.

**Table 7. Pre-moratoria migration rates and informal/formal migration ratio, by province (2008).**

Provinces (ranked)	Informal/Formal migrants ratio	Provinces (ranked)	% Emigration rate (Migrants/Population 15+)
Central Kalimantan	9.00	West Nusa Tenggara	4.24
South Kalimantan	8.47	East Nusa Tenggara	4.01
North Sulawesi	8.23	West Java	2.40
West Java	8.01	Central Java	2.37
Lampung	7.90	Lampung	2.30
Maluku	6.78	South Sulawesi	1.47
East Java	4.59	East Java	1.23
Banten	4.46	North Sumatra	1.04
Central Java	3.71	Banten	0.79
East Nusa Tenggara	2.90	West Sulawesi	0.66
North Maluku	1.67	DI Yogyakarta	0.66
Bali	1.45	South Sumatra	0.65
Bengkulu	1.44	Central Sulawesi	0.37
Southeast Sulawesi	1.18	Southeast Sulawesi	0.34
West Nusa Tenggara	0.77	South Kalimantan	0.24
Gorontalo	0.73	DKI Jakarta	0.19
DKI Jakarta	0.71	Bengkulu	0.19
Riau	0.62	West Kalimantan	0.18
South Sumatra	0.50	Aceh	0.17
Jambi	0.50	North Sulawesi	0.12
Bangka-Belitung	0.47	West Papua	0.11
DI Yogyakarta	0.47	Bali	0.09
Papua	0.45	Maluku	0.08
Kepulauan Riau	0.44	Riau Kepulauan	0.08
West Sumatra	0.41	Jambi	0.05
Kalimantan	0.38	West Sumatra	0.05
Southeast Sulawesi	0.31	East Kalimantan	0.05
West Sulawesi	0.26	Bangka-Belitung	0.03
Aceh	0.16	Riau	0.03
East Kalimantan	0.14	Gorontalo	0.03
West Papua	0.13	Papua	0.02
South Sulawesi	0.08	Central Kalimantan	0.02
North Sumatra	0.06	North Maluku	0.01
Mean	2.34	Mean	0.74
Standard deviation	2.99	Standard deviation	1.12

Note: Emigration rate is defined in per thousand (‰). Own calculations on BNP2TKI data.

### **Appendix 3. Description of *Podes* variables used in the synthetic control analysis**

In all our empirical analysis, we used a large set of variables to predict our labor-market outcomes of interest in the treatment region in the pre-treatment period. For each province we used the information from *Podes* to match the size of “treatment” provinces over time: for this reason, the number of villages, the log of total population and the population density (defined as the ratio between population to total hectares), were included among the predictors.

In order to take into account local labor markets trends across provinces, we also used time-varying information at the local level on: the minimum wage, the structures of local industries across provinces (i.e., on the fraction of villages in which most people are employed in agriculture, mining, industry, retail, transport, services, or other industries), the fraction of urban/rural villages, the fraction of coastal villages, the fraction of households in agriculture, the share of land in agriculture (defined as hectares used for agriculture, as a proportion of total hectares), the share of land in rice fields (defined as hectares of rice fields, as a proportion of total hectares), and the fraction of poor population in the province.

In order to capture the remarkable diversity in the availability of infrastructure across geographic areas in Indonesia, we constructed provincial-level information on: the fraction of electricity subscriber households, the fraction of villages reporting the presence of hospitals, the fraction of villages reporting the presence of maternity hospitals, the fraction of villages reporting the presence of polyclinics, the fraction of villages reporting the presence of community health centers (*Puskesmas*), the fraction of villages reporting the presence of kindergartens, the fraction of villages reporting the presence of primary schools, the fraction of villages reporting the presence of high schools, the fraction of villages where financial institutions were found, and the fraction of villages where a police station was found.

To control for the occurrence of natural disasters, epidemics or conflicts over time as potential push factors for migration, we also used information on: the occurrence of natural disasters in recent years, the number of people who died because of epidemics in the year prior to the interview, the fraction of villages where conflict had occurred, the number of deaths due to conflict, and the fraction of villages where crime had occurred in the past year. Finally, in an attempt to capture political, religious and human capital characteristics across regions, we used information on: the fraction of *kelurahan* villages (where, until the years following the fall of Soeharto, village heads were not elected by villagers), the number of mosques, the number of prayer houses, the number of churches, the number of Hindu temples, the number of Buddhist temples, the fraction of villages where more than one ethnicity is present, the average age of heads of villages / village unit chiefs, the fraction of male-headed villages, the fraction of villages headed by highly-educated heads, and on the enrolment rate in primary school.